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BRAITHWAITE'S RETROSPECT.

VOL. LXXV. JANUARY—JUNE. 1877.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

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LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—*Salicylic Acid.*—The effect of salicylic acid in acute rheumatism appears to depend greatly on the way in which it is employed. I have yet to see the case of genuine acute rheumatism without complication in which the pain is not entirely gone, and the temperature normal, after six consecutive doses of 20 grains, at intervals of an hour, on two successive days. (Dr. W. H. Broadbent, Practitioner, Feb., p. 88.)

Subcutaneous Injection of Cold Water.—Many years ago subcutaneous injection of water was recommended, particularly by Dieulafoy, in rheumatism of the extremities. The injection was inserted in many parts round the affected joint, ten drops being the quantity employed. The results were most remarkable. The pains diminished, and the patients were able to use the affected limbs; nay, even by this treatment alone and after a single operation, the rheumatism was entirely got rid of. Even against muscular rheumatism, sciatica, &c., it has proved to be a means of very great value. (Dublin Journal of Medical Science, March, p. 235.)

DYSENTERY AND INTERMITTENT FEVER.—*Large Non-Emetic Doses of Ipecacuanha.*—In two pamphlets on this subject, Dr. Alfred A. Woodhull records his experience of this treatment in dysentery and intermittent fever. In dysentery, he first quietens the stomach, which should be empty, with opium, and then gives a large dose of ipecacuanha (25 or 30 grains). The recumbent posture and abstinence from food or drink for at least four hours should be strictly enjoined. The dose may be repeated in from two to six hours. He thinks that when the stomach is empty the medicine acts non-emetically in direct proportion to the severity of the attack. Of twenty-three cases of intermittent fever treated by ipecacuanha in doses of from 1 to 20 grains, all were cured but one. Dr.

Woodhull thinks that one or two grains, given every three or four hours, is the best method for ordinary chills. Large doses did not appear to exercise a beneficial effect in proportion to their size. If large doses be used, it is well to guard them with small quantities of opium, and to observe the precautions of recumbent rest and abstinence. (Edinburgh Medical Journal, Feb., p. 764.)

Excessive Sweating.—*Anhydrotics.*—Anhydrotics are remedies which are of use in checking excessive sweating from disease. The sweats of phthisis especially call for these remedies. Phosphoric acid, sulphate of copper, tannic and gallic acids are of some use as anhydrotics. The most potent of all anhydrotics, in my experience, is unquestionably belladonna. We are indebted to Dr. Sydney Ringer for our knowledge of this property of belladonna: and the debt we owe to him can only be sufficiently estimated by those who have an extensive experience of phthisis, and who give the drug a fair trial. I have no hesitation in saying that the use of this agent completely changes the aspect of many cases of pulmonary phthisis. For the arrest of the exhausting night-perspirations of phthisis, belladonna is as potent as digitalis is in giving tone to a feeble heart. It is quite true that neither is very effective in the last and final stages of disease, for indeed nothing is very potent then; but in the early stages the action of each is very pronounced. In the night-sweats of spreading caseous pneumonia, the administration of belladonna is followed in almost all cases by a decided arrest of the flux; and in many cases the arrest of this flux is accompanied by immediate improvement. To produce these effects it is necessary, however, to use larger doses than those spoken of by Dr. Ringer. He speaks of from $\frac{1}{200}$th to $\frac{1}{100}$th of a grain of atropine given hypodermically; and of from $\frac{1}{80}$th to $\frac{1}{40}$th, by the mouth. I have had no opportunity of trying the hypodermic method; but as to the dose given by the mouth, I usually commence with $\frac{1}{75}$th of a grain, and go up to $\frac{1}{25}$th; the latter dose rarely failing. I am inclined to think that in Mr. William Murrel's sixty cases referred to by Dr. Ringer the large proportion of failures (from 8 to 10 per cent.) was due to some extent to his not pushing the drug. When $\frac{1}{75}$th is ineffective, I prescribe $\frac{1}{50}$th; if next week that has failed, $\frac{1}{25}$th is ordered. This usually produces the desired effect, after which smaller doses will maintain it, and may be continued. (Dr. J. Milner Fothergill, p. 43.)

Fevers.—Dr. Sydney Ringer found that the temperature in fevers was increased by the amount of urea excreted, there-

fore give a medicine which will arrest this process. Dr. McBean has tried the cuca with success. In one case he says: Temperature 104; pulse 108; respirations per minute, 34. The amount of urea excreted during the preceding twenty-four hours was 58 *grammes* (2·05 ounces). I prescribed a drachm of tincture of cuca every four hours. From October 10th until October 18th, the tincture was continued, and the dose gradually increased to three drachms every four hours. On the 18th, the amount of urea excreted during twenty-four hours was 28·05 *grammes* (nearly one ounce). With the diminished amount of urea, there was a corresponding diminution of temperature, pulse, and respiration, all of which at this date had become quite normal. In this case, no remedy save the tincture of cuca was used; and, as in the preceding case, from the time the patient began to take the tincture until convalescence was established, there was a daily amelioration of symptoms. (Dr. S. McBean, p. 1.)

Gout.—The phenomena of the acute gouty paroxysm are best explained on the theory that the disease is essentially a capillary phlebitis, the venous inflammation being caused by the circulation of a blood-poison. Now we know that animal poisons of obscure character are occasionally produced in the blood, and cause inflammation of the veins among other evils. Is it not probable that, in gout, the blood, charged with urinary material, becomes so changed, that a poison is elaborated which inflames the venous capillaries? Too little attention seems to have been paid to the amount of solid food allowed to gouty patients. Most of them eat too much. It is much the fashion to prescribe strongly alkaline waters in large quantity. This depresses the gouty, and tends to impair the digestive power of the stomach. In moderation, however, the milder alkaline waters are of value. Though it is true in the general that alcoholic drinks in the form of spirits are better than wines, yet it often happens that these, after a time, become very objectionable to the patient, and prevent him eating his food with appetite. In this case, it is always better to allow wine, which should be always of the highest quality. From what I have observed, I am much inclined to believe that really good wines are generally used to advantage, and that the vegetable extractives of these mellowed specimens are easily digestible by the gouty stomach. Much may be done by remedy to ward off the gouty seizure, and the patient generally receives a warning. When such symptoms arise, a combination of quina, colchicum, and colocynth, taken twice or three times a day, is an excellent medicine. (Dr. G. Owen Rees, p. 39.)

Dilute alcohol, in the form of distilled spirits, has little tendency to produce gout, at least in those not predisposed. Alcohol, when in combination with other substances, as in wines and malt liquors, becomes a potent cause of gout. Neither the acid, sugar, nor any known principle contained in these liquors, can as yet be proved to impart to the alcohol its predisposing influence, for wines the least acid, and liquors the least sweet, are often among the most baneful. Colchicum is held up as a specific, its *modus operandi* being quite obscure, but the author thinks it is not in virtue of its purgative or diuretic action, and he cannot assert that it has any influence upon the excretion of urea and uric acid. (Dr. A. B. Garrod, Glasgow Medical Journal, Oct., p. 521.)

An attack of gout may sometimes be warded off by gentle and equable pressure applied by means of strips of plaster; of course constitutional means are also indicated. The great art in the treatment is to have equable pressure. Any part left without pressure makes the whole part painful. It is sometimes difficult to effect our purpose at awkward angles, or in parts where the malady is deep-seated; but if pain remains I always mistrust the proper application of the plaster. Keep the capillaries empty, and there will be no pain. Whenever the plaster is removed, the skin should look white. As soon as it has been long enough removed for the skin to become red, the pain returns. (Dr. Robert Bowles, Lancet, Feb. 10, p. 223.)

Intermittent Fever.—Instead of using quinine try salicin. In three cases reported by Dr. Thompson it was satisfactory. In one case he says "The paroxysms of ague occurred every three or four days, and generally three times a day. As the next would be his bad day, I ordered him to begin the salicin at once (thirty grains every two hours, if awake). Saw him at 4 p.m. next day. Has had only one fit, and that the powder seemed to stop short; feels and looks much better. To take thirty grains every four hours until after his next bad day; if not well then to continue the medicine until seen again. Visited on the 9th November, 1876. Said he had a slight feeling of chill on the fourth day after beginning the salicin, but yesterday (the 8th) felt as well as he does to-day, and that is better than he has done for six months; in fact, he says his limbs are not so stiff as usual, and he walks much better. (Dr. W. Thompson, p. 62.)

Ptyalism.—How is that "Plummer's pill" very seldom causes salivation? Because of the sulphur contained in the sulphurated antimony. Sulphur in fact is a preventative of ptyalism, but in using it for this purpose success did not crown my efforts

until, by careful observation, I learnt the proper mode of administering it, which is in *small and repeated* doses, *special care being taken to diminish the quantity if relaxation of the bowels supervene;* for its peculiar action in controlling ptyalism depends upon its being retained in the system, and not allowed to pass off by the bowels—which, if necessary, should be prevented by the addition of a few minims of liquor morphiæ or tinctura opii. The bowels should not be moved more than once or twice in twenty-four hours. If persevered in regularly every three or four hours, the secretion of saliva and soreness of the gums become very sensibly diminished in the course of thirty-six hours or less. The form in which I have been accustomed to prescribe it is—℞. Sulphur. præcip. ℈ij ad ℈iv.; potassæ chlorat. ℈ij. ad ʒj.; liq. morphiæ ʒj ad ʒiss; mist. amygdalæ ℥viij. Misce bene et fiat mist., cujus sumantur cochlearia magna ij quâqua tertiâ vel quartâ horâ, phialâ agitatâ. I venture to express my belief that the antidotal action of the sulphur is entirely independent of, though possibly assisted by, the other remedies. With regard to the potass, I at first prescribed the nitrate, subsequently the bicarbonate, and lastly the chlorate, which, being a neutral salt, does not produce the painful smarting of the gums which the two former preparations do. The mistura amygdalæ, in addition to somewhat disguising the nature of the remedy, offers a bland vehicle for its administration. (Dr. Jukes Styrap, p. 56.)

Scarlet Fever and Small-Pox. — *The Use of Peroxide of Hydrogen for the Prevention of.*—Fifty-one cases of scarlet fever came under the care of Dr. Day during a period of two years. They were all treated in a similar manner. Each patient was freely rubbed over the whole surface of the body three times a day with a preparation composed of ethereal solution of peroxide of hydrogen (erroneously called ozonic ether) and pure lard, well incorporated without the aid of heat. The inunctions were continued for about three weeks. No other remedies were prescribed, except in a few cases where the throat symptoms were severe, when a gargle, composed of two drachms of ozonic ether in eight ounces of water, was ordered to be used every second hour. Peroxide of hydrogen contains a larger amount of oxygen than any other known substance, and, moreover, one-half of its oxygen is loosely combined and in a highly active condition, ready to combine with any organic matter with which it may be brought in contact: so that it would appear to be an agent specially suited for the destruction of the poison-germs of scarlet fever, small-pox, and other epidemic diseases. The

following is the formula for the external application. Ozonic ether, four drachms; pure lard, four ounces; benzoic acid, twenty grains; otto of roses, four drops; to be carefully mixed without the aid of heat. The benzoic acid, in addition to its being a powerful antiseptic, possesses the property of allaying cutaneous irritation, a symptom often very distressing to scarlet fever patients. The otto of roses gives an agreeable odour to the preparation. (Dr. John Day, p. 27.)

Small-Pox.—There is no doubt that the great danger of small-pox arises from the suppurative fever which occurs. If this can to any extent be prevented, we gain very considerably in our success. The following plan of treatment should be commenced at the very beginning of the attack. It consists in painting every part of the skin where the eruption appears with one part of carbolic acid dissolved in from eleven to fifteen parts of glycerine, and repeating the application night and morning. The urine must be watched with great care, as it often happens that the carbolic acid becomes absorbed and makes its presence known in the urine by giving the fluid a dark smoky appearance. If this be observed, the application must be made less frequently, or a weaker solution of the acid employed, as it may act too severely as a depressing agent, though I never knew this actually to result from the use of even the more concentrated solution. The employment of carbolic acid in this way has other advantages besides those already mentioned. It acts as a disinfectant, and it prevents pitting to a very great extent. (Dr. Robert Bell, p. 58.)

Prevention of Pitting.—I have used many applications to prevent pitting in cases of small-pox, but none from which I have derived such manifest good results as from common linseed-meal poultices assiduously applied to the face from the moment the eruption shows itself until the fever begins to decline. Among the many advantages that the poultice possesses over other applications, is the by no means unimportant one of softening and determining to the skin, and thus aiding the development of the pustules, and in this way relieving the system of the strain placed upon it. (Dr. Robt. J. Cooper, p. 61.)

Tympanitis in Fever. — It is common in fever, whether enteric or typhus, to have as a complication tympanitis, which may be quite independent of peritonitis or perforation of the bowel. The treatment I have found most useful in relieving this condition is opium. Occasionally the distension of the intestine comes on very suddenly, when it is not only a source of distress and danger, but carries very grave prog-

nostic import. My interpretation of the phenomenon is that it is one of the manifestations of nervous shock, and that it indicates paralysis of the sympathetic system, with consequent loss of tone in the muscular wall of the bowel, allowing the distension to take place. I do not hesitate to give and repeat a drachm of tincture of opium. I can recall to mind many instances in which by this treatment the tympanitis has been dissipated in a few hours, with a corresponding improvement in the general condition of the patient. (Dr. W. H. Broadbent, Practitioner, Feb. p. 89.)

Typhoid Fever.—*Antipyretic Treatment.* — Observers have found, that after frequently-repeated baths, commencing with a temperature of 68° F. (thermometric observations being regularly recorded), there is a decided decrease in temperature, and a marked remission of the more serious symptoms. In an epidemic such as I have described, this mode of treatment can never become general (except, perhaps, in the case of children), both because of the want of suitable accommodation and appliances, and from the difficulty of obtaining qualified intelligent nurses to carry out the treatment prescribed. I lately became aware of the antipyretic effects obtained by Liebermeister and Vogt after the employment of large doses of quinine. In one patient—the first so treated—the disease had reached the middle of the second week when I found an evening temperature of 105·5° F., accompanied with delirium, catarrh of the smaller bronchi, and frequent cardiac action. With no little hesitation, I prescribed 45 grains of the sulphate of quinine, in divided doses of 7½ grains every ten minutes, and anxiously awaited the result. The following morning the temperature had fallen to 100·3° F.; the brain acted normally if slowly; and the pulmonary affection had almost entirely disappeared. Although the temperature subsequently rose, it never reached the same height, and the patient made a good recovery. Convinced, then, of its utility, I have employed it in a considerable number of severe cases, with the result (with very few exceptions) of reducing a temperature of 104°-105·5° F. to 100° F., and even to 98·5°; and with this decrease, a marked improvement in the general symptoms. To obtain such a result, large doses must be given at brief intervals,—or it may be given in one single dose—for quinine is rapidly eliminated by the kidneys. For adults, I order, as recommended by Liebermeister, 45 grains divided into 6 powders, one of which is to be taken every five or ten minutes. Where I have considered the effects of this dose insufficient, I have ordered as much as 50 grains, without observing any bad

effects beyond the physiological action of the medicine, and perhaps vomiting with the first or second dose. In addition to the ringing in the ears and increased deafness, I have observed in several cases, especially in young people, marked dilatation of the pupil, and in nearly all, general paleness of the cutaneous surface. For every case which threatened to be at all serious in the course of this epidemic I had a chart drawn out, on which the temperature and frequency of pulse, number of stools, &c., were carefully noted at the bedside, morning and evening. In most cases, when the temperature reached 103·5° F. in the morning, or 104·5° F. in the evening, quinine in antipyretic doses was administered. Latterly, however, finding that a temperature slightly lower, but more continuous, produced equally serious threatenings of cardiac paralysis, as evidenced by increased frequency and weakness of pulse, and hypostases of the lungs, I had recourse to the remedy even before this height was reached. Whether it be administered in the morning, as suggested by Binz, or in the evening, as recommended by Liebermeister, appears to be immaterial, for, at my next visit, a marked reduction of temperature was always sufficiently evident. More than one dose was always required; but it was seldom necessary to repeat it oftener than once in two days, and frequently not so often. We have thus in quinine a potent and trustworthy remedy, possessing nearly all the efficacy of the cold bath, and yet almost universal in its application. The use of salicylate of soda in this disease seems suggested at once by its remarkable action in rheumatic fever. Like quinine, it must be given in full and frequently-repeatedly doses. For an adult, 10 to 20 grains are given every ten minutes until 6 doses are taken. For a child of 5 years I have given as much as a drachm and a half in an hour without any toxic effects resulting; but in two instances—both children—slight croupiness of inspiration followed, probably the result of catarrh of the pharynx and upper part of the larynx. Abundant diaphoresis has attended its employment in two or three of my patients. Tried at intervals of one or two hours, no antipyretic action was noteworthy, proving that it is rapidly eliminated. In nearly all the cases treated with salicylic acid the pulse fell with the temperature, delirium of a mild character gave way. Symptoms of pulmonary œdema and hypostatic congestion became less anxious complications, and in these respects I found a decided resemblance to the action of quinine. Salicylic acid is, moreover, a valuable astringent, and is therefore peculiarly indicated in those cases where the diarrhœa is frequent and exhausting. (Dr. George Hunter, p. 16.)

AFFECTIONS OF THE NERVOUS SYSTEM.

ACUTE MANIA.—*Action of some Alkaloids as Soporifics.*—In a case of acute mania we felt unless we could give the patient sleep she must soon die, and, as morphia had failed, we determined to use hyoscyamia. On the evening of October 25 we gave her a grain of the crystallised alkaloid, with most complete success. We then resolved to give atropia, and see if that would answer as well. We were afraid to give a grain of atropia, so on the first night we gave only a quarter of a grain, but without producing any sleep. We next gave three-quarters of a grain, and then a grain, and afterwards we employed daturine and ethyl-atropia. We find that hyoscyamia on an average produced $9\frac{1}{2}$, sulphate of atropia $7\frac{1}{4}$, and daturine $7\frac{1}{4}$ hours' sleep. It thus appears that in cases of madness or delirium, sulphate of atropia and daturine are as good, or nearly as good, as the far more expensive alkaloid hyoscyamia. (Dr. Sydney Ringer, p. 119.)

ACUTE TRAUMATIC TETANUS.—*Choral Hypodermically Injected.*—A case of acute traumatic tetanus is related in which recovery resulted from the hypodermic injection of chloral. The treatment consisted in wearing out the acute character of the disease by the continued exhibition of the drug; neutralising the tetanic poison, so to speak, as fast as it was secreted; or, in other words, de-polarising the nervous centres excessively charged by the morbid processes of the disease. This is to be done by injecting five, and even ten grains of chloral in five minims of water, in the arms or legs, almost on every attack of spasm, and continuing the practice for several days. (Mr. J. H. Salter, p. 92.)

CHOREA.—In severe and acute cases, where the patient is being worn out by incessant movement and want of sleep, liberal feeding, stimulants, and the means of procuring timely slumber—the bromides, opium, or chloral—may enable him to tide over a period of mortal peril. Purging does distinct good, and sometimes is the only medicinal process needed. Many metals—antimony, arsenic, iron, and zinc—markedly influence the disease in question; antimony perhaps controls the jactitation of severe and recent chorea in the most immediate manner, but it must be given largely to be effective, and so used it adds to the prostration of the patient, and sometimes, I believe, is the chief cause of a fatal result. Zinc stands next in the order of efficiency. To be of use it must be given in large doses. A grain of the sulphate may be given three times a day, or in a very severe case more often, and a grain added to each dose every day until the dose amounts to between fourteen and twenty-six grains. Thus

administered and sufficiently diluted it causes no sickness nor any prominent effect but the abatement of the jactitation and grimace. A scruple or rather less is commonly a sufficient dose, but much more may be given. In an exceptionally severe case of which the subject was a girl of seven, I gave with apparent advantage, and certainly without harm, a dose which at last reached forty-five grains three times a day, or 135 grains in the twenty-four hours. Under this the child became able to talk, feed herself, and walk, none of which she could do before. With the subsidence of the chorea, the zinc may be gradually withdrawn, and the iron at last continued alone, or with the addition of quinine. Another salt of zinc, the valerianate, is of especial use; it is suited to cases of a less acute type than to require the sulphate, and to those by no means infrequent instances in which the attack has with it some of the characters of hysteria. Next to the salts of zinc, and often to be preferred to them, come those of iron. Where there is evident anæmia, iron in some shape should be given from the first. Zinc does best with florid children, iron with the pallid; zinc when the symptoms are acute, iron when they are chronic. (Dr. W. Howship Dickinson, p. 105.)

Chloral Hydrate.—This disease is naturally suspended during sleep, and therefore a drug which produces sleep is plainly indicated. For this purpose no drug is so suitable as chloral —but it will be actually injurious if not administered so as to get its narcotic effect. The principle of treatment in each case was to administer the chloral so that the patient should get about ten hours' sound sleep in the twenty-four. With this intent a dose of gr. xxx. was administered, and this would have been repeated, as much, or half as much, or less, according to the circumstances if the patient had not slept. Then immediately on waking a second dose was given, in proportion to the ascertained effect, less or more as the patient had slept well or ill, but in any case less than at first: and on waking again, another dose less than the second, and so on till the amount of sleep required had been obtained, when the chloral was discontinued till the next night. Of the two patients thus treated, one was cured completely in one day; the other had a few choreic movements only left on the third day. In each case I gave a preliminary dose of gr. xv. to discover any idiosyncrasy. (Dr. Robert Bridges, p. 108.)

Strychnia.—In a case of chorea without history of rheumatism or cardiac complication, on 15th May, I commenced the administration of strychnia in doses of $\frac{1}{60}$th drain of the sulphate three times a day. On 17th the dose was increased to $\frac{1}{48}$th grain, and each day it was increased little by little.

The prescription I adopted is one recommended by Hammond, which, by its accuracy and the facility of regulating the dose, supersedes that of Trousseau. A solution of sulph. strychniæ, of half the strength of that of the British Pharmacopœia, is made, so that five minims shall represent $\frac{1}{48}$th grain of the alkaloid. Sufficient is ordered for one day, and syrup added. Each day an additional minim of this solution is added to the dose of the day before, so as gradually to reach the dose which will produce the physiological effects of this powerful excitant, or which will suffice to check the excited movements of the muscles. In the case before us I employed, in addition to the strychnia, what seems to me to be a useful adjunct—namely, the application of the ether spray to the spine, from the nape to the sacrum. In four or five days from the beginning of the treatment when the dose of the strychnia had reached only $\frac{1}{30}$ gr. ter die—there was marked evidence of improvement. The patient had much more command over the muscles of his hand, so that by an effort (of certainly no long duration) he could keep it steady, and open and shut each finger in succession. He left hospital, seemingly quite well, having been under strychnia treatment fourteen days. The highest dose of strychnia reached in this case was $\frac{1}{17}$th grain. It, however, produced none of its physiological effects, such as pains and stiffness in the neck, arching of the back, or cramps and pains in the legs. The only marked effect seemed to be the production of an enormous appetite for bread, of which the patient devoured, in addition to his allowance, an extra medium-sized loaf per diem. (Dr. J. Magee Finny, p. 98.)

Epilepsy.—*Bromide of Sodium.*—Dr. W. A. Hollis records eleven cases of epilepsy in which most favourable results were obtained with bromide of sodium. He gives from 15 to 40 grains of the salt in an ounce of camphor water, or other vehicle, thrice daily. For a child two years old, he advises the administration of three grains in two drachms of decoct. cinch. pallid. three times a day. Dr. Hollis observes that the salt appears to exert a greater influence on the so-called "convulsions" of children than it does on the more confirmed epilepsy of adult age. (British Medical Journal, July 1.)

Nerve Sedative.—*Bicarbonate of Potash.*—At present, we hear much of the relative value of the treatment of rheumatic fever by the use of salicine or salicylic acids and its compounds, and that where the alkalies or the acetate or bicarbonate of potash are used. That the bicarbonate or potash is a nerve-sedative can easily be demonstrated by anyone taking a large dose of it, such as a drachm, in water. A

peculiar tingling, numbing, sensation is soon felt in the lips and cheeks, and afterwards in various parts of the body, extending gradually downwards to the lower limbs. I have used this drug, combined with the bromide of potassium in epilepsy, with decided effect. That this sedative action on the nerves has a great deal to do with the successful treatment of rheumatism by the bicarbonate I have not the slightest doubt. As this is a fact, it is desirable, in treating rheumatic fever with this drug, to give it in sufficiently large doses, such as a drachm dissolved in water, every few hours. That some cases are best treated by means of alkalies there can be no doubt. Others, however, and especially those where there is great hyperpyrexia, are better under the salicine or salicylic acid treatment. (Mr. Jno. A. E. Stuart, British Medical Journal, Dec. 9, p. 750.)

NEURALGIA.—*Croton Chloral.*—Liebreich has shown that this drug is essentially an anæsthetic, and in large doses a hypnotic; that anæsthesia always begins at the *head*, and only after this region is fully under the influence of the croton-chloral and sleep has been induced does the anæsthetic effect gradually extend over the rest of the body. With large doses the respiration and pulse become slower, and a fatal result is due to arrest of the *respiratory* movements, not to paralysis of the heart; in this respect croton-chloral differs from chloral, as with the latter death occurs directly from cessation of the *heart's* action. We should from the above be led to expect that the most marked effects of croton-chloral as a therapeutic agent would be seen in the case of painful affections of the *head*. I have prescribed it in cases arranged under the following divisions. 1. Pain or other abnormal sensations about the face, head, and neck, including—(*a*) Ordinary headache, affecting the anterior part of the head, or the occiput and upper part of the back of the neck. (*b*.) Megrim, migraine, or so-called bilious sick-headache. (*c*) Facial neuralgia and toothache. (*d*) Headache associated with cerebral disease. (*e*) Sensations such as giddiness without actual pain. 2. Pain in other regions, comprising—(*a*) Pain other than visceral, in the limbs and trunk. (*b*) The pain of dysmenorrhœa. (*c*) Pain connected with digestion. 3. Hysterical convulsions. The dose in each case was five grains three times a day. (Dr. E. Markham Skerritt, p. 83.)

Dose and Mode of Administration of Croton-Chloral.—Croton-chloral is but sparingly soluble in water, but to a sufficient degree to make a solution as strong, in my opinion, as any patient will be likely to take, as it is the reverse of palatable. Ten grains will dissolve in the ounce of water without much

difficulty; glycerine makes it rather more soluble. Dr. Yeo gives four grains to the drachm as the strongest solution that can be made with water and glycerine. At the hospital Mr. Berry uses an alcoholic solution. The drug may also be administered in the form of pills. There does not seem to be any risk from large doses of croton-chloral. Dr. Ringer has given five grains to a patient every hour for a fortnight, and Dr. Liebreich has prescribed a sleeping draught containing about a drachm and a half. In hospital out-patient practice I generally order five grains three times a day, and have found that patients unrelieved by this dose are often very much benefited by the addition of another dose of five grains per diem. Elsewhere I have given the same dose every two, three, or four hours, according to the urgency of the case. The only unpleasant effects I have observed have been the following. In two cases, vomiting; in one of these this was so constant after the dose that the medicine had to be discontinued; in the other it ceased when the drug was taken immediately after meals, and the desired effect was obtained. In several cases, drowsiness, but not so great as to necessitate the discontinuance of the remedy. In two or three cases, giddiness; in one, headache. From the details that I have given it will be seen that in my experience the results of the therapeutic use of croton-chloral have been such as might be anticipated from a consideration of its physiological action; as the primary effect is shown in producing anæsthesia of the head, and only after this is well marked does its influence extend to other regions, so my experience has been that its remedial value is almost limited to painful affections of the head. (Dr. E. Markham Skerritt, p. 90.)

Pain.—*Outward Application of Hydrate of Chloral.*—Hydrate of chloral is capable of cutaneous absorption, and acts in this way as a local anæsthetic. I have tried it with great success in neuralgic pains and in cancer of the breast, in cases in which other sedatives and narcotics have failed to give relief. The mode of application is by the saturation of folds of lint of the size of the part to which it is to be used, brought into close contact, then covered with three or four layers of lint, covered with oil-silk or spongio-piline wrung out of hot water. The application to raw surfaces, of course, requires some care in manipulation. The strength of the solution is about four drachms to sixteen ounces of water. The addition of a small quantity of glycerine is advantageous. Chloride of zinc or perchloride of iron can be combined with the chloral in certain cases. (Dr. W. B. Kesteven, Lancet, Feb. 10, p. 204.)

Sciatica.—There are three methods especially directed to the relief of pain. Hypodermic injections of morphia doubtless hold the first place. For my own part, I prefer employing very moderate doses, beginning with gr. $\frac{3}{30}$, and rarely finding it necessary, except in malignant disease, to exceed gr. $\frac{8}{30}$, a dose which is only arrived at by degrees. The solution I employ contains gr. i in ℥ xxx., which is much more convenient than the pharmacopœial strength for giving small doses, and appears to me to be more active in proportion than the more concentrated form. The side of the buttock is a convenient place for the injection, which should be repeated, I think, as the effect begins to wear off. In very severe cases twice in the twenty-four hours is usually sufficient; when the disorder is less acute once a day is often enough. Flying blisters in my experience are frequently of extreme service. They need not be larger than a five-shilling piece, and one should succeed another as it heals, not on the same, but on a closely adjoining part of the skin. The *constant voltaic current*, on the other hand, may be employed in this class, not only with results which are often strikingly palliative of suffering, but which are also sometimes curative, it may be rapidly, but more frequently after a considerable number of applications. Any good constant-current battery may be employed—Dr. Tibbits' arrangement of the Leclanché, and Dr. Stöhrer's modification of the Bunsen, are probably the best for medical purposes,—and it will be necessary to use a current which proceeds from a tolerably large number of cells, say from about twenty to forty. The strength of the current, when the full number of cells is gradually arrived at, should be as much as the patient can bear without much inconvenience; it should be very distinctly felt, however. As regards the duration and frequency of the applications, it is generally found that from ten to fifteen minutes is sufficient. The sitting should be repeated daily, or even more than once daily. (Dr. Thomas Buzzard, p. 74.)

Oil of Turpentine.—Turpentine is a valuable medicine in sciatica. In a number of cases the following mixture was directed to be taken early in the morning:—℞. Ol. terebinth. ʒ ij.; ol ricini ʒ iv.; tinct. cardam. co. ʒ i.; mucilag. et aq. ad ʒ ii. In ten cases relief from pain was immediate, not in any entire or complete after a single dose, but such an amelioration as to render lying on the side affected, or even sitting up, quite possible. A second or third, in several even a fourth dose, was required to reduce what was previously intense suffering to a state of slight numbness. Turpentine, then, to do good in the sciatica of the period of tissue-degeneration, must be given in doses of at least two drachms,

combined with half an ounce of castor-oil, suspended in mucilage, and rendered more palatable by the addition of an ounce or so of cinnamon-water. The draught is best taken early in the morning. The dose generally acts powerfully two or three times, but its action is not followed by exhaustion at all commensurate with its effect on the bowels. (Dr. W. Allan Jamieson, p. 80.)

Nervous Debility and Spinal Irritation.—Mild and continued counter-irritation of the back is sometimes of the greatest benefit in this class of cases. The preparation which I have almost exclusively employed to produce counter-irritation of the skin of the back has been the linimentum sinapis co. of the British Pharmacopœia. This preparation owes its activity both to the essential oil of mustard and to ethereal extract of mezereon. If properly prepared a few drops of linimentum sinapis sprinkled over a pad of cotton-wool ten or twelve inches long and four or five inches broad will suffice to produce in a few minutes pretty intense redness of the skin of the back, accompanied by more or less of the painful burning sensation characteristic of mustard. The general result of the use of this method of counter-irritation may be very shortly described. On the first or second day of the treatment, the patient, if a delicate hysterical girl, may complain that the pain caused by the mustard is almost unbearable, and she may declare that the application cannot be continued. By diminishing the amount of mustard-oil used, however, all such urgent objections on the part of the patient are removed. As soon as the application has been so controlled as to bring on merely an active glow and not unpleasant tingling of the skin, the patient declares that the increase in her strength is marvellous; the pains in the back and limbs undergo a diminution, or, as long as the mustard counter-irritation is kept up, are completely in abeyance, the irritability of temper diminishes, and simultaneously the general health undergoes marked improvement. (Dr. Arthur Gamgee, p. 64.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Aneurism.—I wish to submit to the consideration of surgeons a simple suggestion for the safe and rapid cure of aneurism. It is, to stop the circulation above and below the aneurism, and substitute for the fluid contents of the sac a substance insoluble in blood, solid at the temperature of the blood, fluid at a temperature low enough to allow of its being safely brought into contact with living tissues, and changing from liquid to solid without fail and with great rapidity, and which

at the same time is light, innocuous, and unirritating. All these conditions are completely answered by either spermaceti, melting at 120 deg., or stearin, melting at 130 deg.; and I submit to the consideration of surgeons whether there is any practical reason why an aneurism should not have its fluid contents withdrawn by an aspirator and their place filled by melted spermaceti or stearin. Either of these substances would so rapidly and permanently solidify *en masse* as to be absolutely free from the danger inseparable from either "active" or "passive" clots being washed away when the blood-current is again allowed to flow: and the time occupied in their solidification would be so short as to remove all danger of damage from arrested circulation in the parts below the aneurism. I need scarcely add that the subsequent blocking of the artery above and below the aneurism will, of course, go on as usual. (Dr. Horace Dobell, Brit. Med. Journal, Feb. 3, p. 134.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

CATARRH.—*A Cold in the Head.*—This treatment refers to that particular form of "cold" characterised by excessive defluxion from the nares and lacrymation, and more or less febrile disturbance (and not to that which, in ordinary language, is styled a "a dry and stuffy cold"), and is based on the principle of restoring the natural functions of the skin, which a chilling wind or other atmospheric influence on persons with lowered vitality has wholly or partially suppressed. I invariably give *small doses of morphia and antimony* every three or four hours until the sneezing and defluxion cease, which, with ordinary precaution, results after the third or fourth dose. The antimony has, in my opinion, a more special effect on the mucous membrane of the breath-passages than ipecacuanha. The following are the forms which, slightly varied, I have used for many years: a dose or two of either of which has enabled me on various occasions, when suffering from catarrh, to attend to my professional duties with comparative impunity. Confinement, however, to the house for a day or two should, I need scarcely remark, be insisted on, whenever practicable. The warm or hot air-bath (or "packing"), as suggested by Dr. G. Johnson, is a valuable adjuvant to the treatment, if had recourse to on the day of seizure; and, in severe cases, I generally recommend one or the other, if obtainable, and an immediate retirement to bed in a warm room. ℞. Liq. morphiæ (*P.B.*) ♏xl; vini antimon. ♏xxx: potassæ citratis ℈iv; syr. aurantii ℥iij; aquæ ad ℥iv. Misce et fiat mistura, cujus sumat cochlearia

magna ij quâqua tertiâ vel quartâ horâ. ℞. Liq. morphiæ ♏xl; vin. antimon. ♏xxx; liq. ammon. citrat. ℥j; potassæ citratis ℈iv; sp. chloroformi ʒj: aquæ ad ℥iv. M. Ft. mist. cujus capiat cochlearia magna ii quâqua tertiâ vel quartâ horâ. (Dr. Jukes Styrap, p. 47.)

The Dry Treatment.—This plan may be widely known, but is not so much appreciated as it deserves. It was first described by Dr. C. J. B. Williams. It has been called the *dry plan*, and consists simply in abstinence from drink of any kind until a cure is effected. The theory upon which it is based cannot be better expressed than in the words of Sir T. Watson: "The principle here concerned is that of cutting off the supply of watery materials to the blood. The wants of the system exhaust from the circulating fluid all that can be spared for the sustentation of the tissues, or for the natural evacuations, and there is nothing left to feed the unnatural secretion from the inflamed mucous membrane. Its capillary vessels cease to be congested; the morbid flux is diverted, and the inflammation starved away." 1. Begin with a sharp aperient in the solid form of pills, swallowing them with as little water as possible. 2. The food should be rather less in quantity and more digestible than usual, and at first should be dry; later on, the moister forms of food are more easily swallowed and digested. 3. As much exercise as possible should be taken in warm clothing, to promote the action of skin and bowels. (Dr. W. J. Haram Wood, p. 50.)

A well-known treatment of catarrh, or of the early stage of bronchitis, is to give acetate or citrate of ammonia with perhaps free ammonia, vinum ipecacuanhæ, spirit of nitrous ether or of chloroform, and some preparation of opium. One can only give a certain amount of such a mixture at a dose, but by giving a series of doses, say at eight, nine, and ten p.m., instead of every three or four hours, we may often cut short an attack which would otherwise go on for days. (Dr. W. H. Broadbent, Practitioner, Feb., p. 89.)

Empyema.—Instead of using the aspirator, trust to a free opening, and the carbolic acid and salicylic acid dressings. Dr. Bennett says,—In the case which I have recorded, I found that it was necessary that the air should be allowed to flow freely in and out of the pleural cavity with each act of respiration during the entire period of treatment, a condition not present in any other wound I have treated on this plan, and one which tests to the utmost the powers of the antiseptic used. In the early part of the treatment I applied very wide gauze dressing, secured by a few turns of a broad binder. Subsequently, when the discharge threatened to wash away

the carbolic acid by its excessive flow, I added a pad of salicylic jute to provide an antiseptic which should be capable of saturating the discharge as it flowed through and outside the bandages. Subsequently, I gave up the use of carbolic acid spray, to change the character of the application to the pleural surface; and lastly, as the gauze, worn for a long time on the skin outside the pad of jute, caused irritation and severe itching, I substituted for it cotton wool, in the unbroken sheet, saturated with salicylic acid, secured to the side by having ordinary soap plaster spread around the edge of the piece. In this way I was gradually led, as the case progressed, to use only the preparations of salicylic acid in the dressing, and these were constantly worn during the termination of the case, while the patient moved about the house, and lastly walked and drove in the open air. During the entire treatment no trace of decomposition or of fœtor occurred. The pad of jute, prepared after the method of Thiersch, with, on its outside, either a carbolic gauze dressing or an unbroken sheet of cotton wool, charged with salicylic acid, provides a perfect filter for the air necessarily drawn through it into the chest—a property due largely to the moist condition of the jute, in consequence of its being prepared with glycerine. The original object of the addition of the glycerine in such quantity as to keep the fibres moist (4oz. to 1lb. of jute) was to prevent the crystals of the dry acid flying off the jute in dust as it was handled; but I am confident that the glycerine fulfils the further object I have stated, rendering the jute a perfect air filter. A trivial, but still not entirely unimportant point, I have learned also in the management of this case—namely, a ready mode of fixing the drainage tube in such a case, where but one small opening exists, and the risks of escape of the tube either into the cavity or completely out of the opening, as is liable to happen with the constant respiratory and other movements, must be avoided. After various plans had been tried, I adopted, as the readiest and most manageable, a single piece of salmon fishing-gut, carried as a suture through the tube at right angles to its axis, and cut off a couple of inches from the tube at either side. A little piece of isinglass plaster fixes each whisker, as one may name the ends of the gut, to the chest-wall, wet with the spray, and retains it perfectly; the gut is stiff enough to keep the tube adjusted without galling the side, as a wire suture would; and, on the other hand, it does not conduct the discharge to the plaster as silk or thread does, and so prevents its softening and the consequent slipping of the fastenings. (Dr. E. H. Bennett, p. 146.)

HOOPING-COUGH. — *Croton-Chloral.* — I do not know if the administration of croton-chloral in hooping-cough has any originality; but, after a somewhat extended trial, I regard it as almost a specific. I have given it in gradually increasing doses, commencing with one grain twice or thrice a day, and almost without exception there has been a very marked reduction both in the violence and the frequency of the attacks. (Dr. Wm. Paulson, Lancet, Feb. 10, p. 222.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

INTERNAL PILES.—Internal piles may be treated successfully by the following simple operation:—The piles being well down, they are to be punctured to their bases with the conical pointed end which I have had made by Messrs. Mayer and Meltzer to fit on to Dr. Paquelin's gas cautery (see figs. at pp. 351, 353). The number of these hot punctures varies with the number and size of the piles, a pile the size of half a small walnut requiring two or three. A dull-red heat should be used, and the point gently rotated while being extracted, and *not pulled out*, because if this be done a portion of the eschar will be withdrawn with the instrument, and some hemorrhage will follow. Should the disease be of old date, some of the piles will be quite hard; these I have pierced to their softer attachment, at the feeding veins of which they were clot-laminated, and even fibrous varicose transformations. Ulcers and fissures in connexion with the hemorrhoids were touched with the cautery. If this simple plan be properly followed, there is no hemorrhage, but should there be slight oozing, a touch of the cautery at once stops it; the piles are then returned, and a half-grain morphia suppository introduced. The bowels are kept confined by a quarter of a grain of morphia daily, by mouth or subcutaneously, for the first two or three days, and on the fourth or fifth day an enema-tube is gently introduced and a warm injection given, and followed on the succeeding day by a laxative. The first two, or in some few cases three, motions, produce pain, but nothing as compared with that the patients suffered *before* the operation; and at the expiration of a week they are discharged, with such directions as to diet and regimen that will promote the healthy functions of the rectum, and which are known to all professional men. (Mr. H. A Reeves, p. 261.)

RECTUM.—*Introduction of the Whole Hand into the Rectum for the purpose of Diagnosis.*—The hand, if small, can be introduced into the rectum of both male and female without fear of rupture of the sphincter, or incontinence of fæces. The dila-

tation of the sphincter should be very gradual, five minutes at least being allowed for its accomplishment. No pain or inconvenience is experienced by the patient as an after-result of the operation. When once through the sphincter, the windings of the gut should be followed very cautiously by a semi-rotatory movement of the hand, and by alternate semi-flexing and extending the fingers. In many cases the hand can be passed into the sigmoid flexure, and possibly, in rare instances, into the descending colon. Should the hand meet with a feeling of constriction about the junction of the first and second pieces of the rectum, no force on any account should be used to overcome it, as this can only be accomplished by rupturing the peritoneum, which is here reflected from the intestine. This method of investigation is of use in detecting a stricture high up the rectum or in the sigmoid flexure of the colon, but a stricture below the descending colon may exist although the hand may be unable to discover it. (Dr. W. J. Walsham, p. 238.)

Tapeworm.—*Turpentine.*—Of all tænicides the old-fashioned one, oil of turpentine, is the most potent. But turpentine in large doses is not free from inconvenience, or even danger to life; I know of one death caused by its injurious action on the kidneys. This danger can be greatly diminished, if not altogether averted, by the precaution of giving a purgative, if the turpentine does not itself very speedily act upon the bowels. The best plan is to direct the patient to take the following draught, and, if it does not purge, a dose of castor-oil soon afterwards :—Spir. terebinth. ʒ iii; t. hyoscy. ʒ ss; mucil. acac. ℥ ss; aquæ chl. ad. ℥ ii. Free evacuations, containing the worm, together with the long-sought-after head were soon caused by this draught. (Dr. Arthur Leared, p. 159.)

AFFECTIONS OF THE URINARY SYSTEM.

Bile Pigment in Urine.—*New Test for.*—It was first discovered by M. Marechal that tincture of iodine is a most sensitive test for bile in urine, giving with it a magnificent emerald-green colour, which lasts for half an hour, and then passes through rose colour into yellow. The action is invariable. The best mode of procedure is to place about ʒ i. of the urine in a test-tube, and then to allow one or two drops of tinct. iodi (B.P.) to trickle down the side of the tube, held nearly horizontally, so that the two fluids may touch but not mix. If bile pigment be present, a fine green colour will almost immediately be developed below the red layer of iodine tincture. By holding the test-tube up against a white

cloud, or against any white surface, in a good light, the three zones of colour will clearly appear—viz., the red iodine layer, and the yellow stratum of urine, separated by the green layer. The test succeeds better by flotation in a test-tube than by allowing the tincture of iodine to run into contact with the urine on a white plate. If the colour of the urine be very dark, it is better to dilute it with at least an equal volume of water before applying the test. Should the quantity of bile pigment be very small, it is advisable to make use of a specimen of healthy, non-bilious urine as a standard of comparison, treating it similarly with the iodine solution. (Dr. W. G. Smith, p. 160.)

Calculous Affections.—Sir H. Thompson does not approve of alkalies. He says: "Now, it is quite true that with alkalies, provided enough be taken, these deposits (urates and uric acid) will disappear; the uric acid will no longer be deposited; the urine will become less irritating; the annoying symptoms will be diminished or got rid of; and, of course, the patient is very much pleased with this new condition of clear urine and disappearance of all deposit. And you will say, 'What more can be desired?' This: you have merely made his enemy disappear, but he is by no means rid of its presence. You have not checked the acid formation. The uric acid is there as much as ever; but the uric acid and the urates are soluble in alkali, and you have only made them invisible. You really have the same condition as that of the fabled ostrich, which is said to put its head in the bush when pursued by hunters, and, no longer seeing them, to believe itself secure." Not only this, but Sir Henry Thompson believes that large quantities of alkali habitually taken act injuriously, and tend possibly to increase the tendency they are given to check. It is upon attention to diet and to the employment of "such agents as will stimulate the excretory action by the primæ viæ without depressing vital power," that he relies in the treatment of the uric-acid diathesis. The best agents of this kind he believes to be the various purgative or aperient mineral waters, such as Friedrickshalle, Püllna, Carlsbad, and Hunyadi János, and, instead of sending a patient suffering from gravel to Vals or Vichy, he would recommend a course at Carlsbad. But, doubtless, Sir Henry Thompson is conferring a great benefit upon patients suffering from these affections when he points out that a course of these waters, if taken at home, combined with judicious diet and abundant fresh air and exercise, will do as much good as if taken on the spot, and probably more than if drunk in the enormous quantities usually recommended at the watering places. (Sir H. Thompson, p. 273.)

BRIGHT'S DISEASE.—In Bright's disease the poison in the blood causes arterial tension; and the degree of the tension is in proportion to the amount of the poison. In the treatment we have to consider the means of lessening the amount of poison in the blood; the not adding to the blood fresh poison, and the stirring up of the powers of life so that they may the better resist and live above the poison that is there. Soft water, repeated frequently through the twenty-four hours, is the great and effectual diuretic. It is the needful medium for other diuretics if we would really make them do their work. Hard water certainly passes less freely through the kidneys than soft water, and in going through them it carries off much of the poison that has accumulated in the blood. Schweppe's Malvern soda-water, the gas being previously stirred out with a spoon, is the best means that I know of giving soft water. It has the advantage, too, of lessening the acidity of the urine, for acid urine irritates the kidneys and makes them deny their function when affected with Bright's disease, especially in the inflammatory forms; just as acid urine irritates the bladder when it, the prostate, or the urethra is affected. This leads me to say that alkalies, in proper quantity, are as useful to the physician in Bright's disease, as to the surgeon in disease of the bladder and urethra; and for the same reason, since they render the acrid fluid bland and soothing that is constantly distilling through the bladder of the affected kidney. The smallest vessels then cease to shut themselves up, and the tubes are flushed by the abundance of soft mild fluid that is passed through them. We all know how admirably the potash salts often fulfil the purposes of which I have just spoken in these cases. The main motive of our treatment should be the withholding, as far as possible, every article of food that contains the same kind of poison as that which already contaminates the blood; and especially that we should not ourselves actually direct the patient to take such food as his proper nourishment. In carrying out this principle, it is needful that we shall keep steadily before us the character of the poison so infecting the body. The poison contained in the blood in cases of Bright's disease is composed of the materials metamorphosed from the muscles and other tissues, that accumulate in the blood because they are not sufficiently expelled by the diseased kidneys. Beef-tea and soups, deducting albumen, and Liebig's essence of meat, which contains no albumen, are entirely composed of the metamorphosed structures of muscle, including kreatin, kreatinin, and the fatty and aromatic acids. Some of those ingredients are innocuous, but the more important of them essentially cor-

respond with the retained self-generative poison already circulating in the blood. By giving such patients beef-tea or the essence of meat or soups, we are adding the metamorphosed structures of the muscles of the cow to the retained metamorphosed structures that are already poisoning their blood. Beef-tea, soups, gravies, chicken broth, animal fluids of all kinds, essences of meat whether Brand's admirable kind, or Liebig's essence, which, because it is stale, should never be used where fresh beef-tea or essence is to be had, ought to be rigorously forbidden to every patient with Bright's disease. But in this case, in objecting to the use of beef-tea, essence of meat, and the like, with life to be supported by food, it is a duty that cannot be escaped from to find a substitute for the poisonous beef-tea; and in milk, abundantly supplied, we have a perfectly innocuous food that supplies every part of the body with its needed materials of repair; that has already built up and formed every one of us in infant life; that promotes the action of the kidneys, and so tends to expel the poison from the blood; and that may be gratefully mixed with Schweppe's Malvern soda-water, or Apollinaris, or lithia, or other waters, with the effect of enhancing the purifying power of the kidneys and so washing out the poison. Milk, however, two or three or more pints daily, may be given in any form that is agreeable to the patients, cold, iced, hot, with rice, with bread, with flour or vermicelli, or corn-flour, with maccaroni, with fruit, with puddings and tempting well-flavoured dishes of all those varieties that the thoughtful, handy, artistic cook of taste and affection knows so well how to prepare. The question must here be put and answered, whether it is proper or not to give meat to patients affected with Bright's disease. In cases of fatty, lardaceous, and granular kidney, meat in moderate quantities, and properly cooked, is in a practical point of view of unquestionable service. It gives them a compact non-distending food; it helps to sustain their muscular power and action, just as it helps to sustain hard labour, and it gives zest to life. Nor is this result of practice otherwise than confirmatory, as it always must be, of the physiological view of this question. Meat supplies the body with the exact ingredients of the patient's muscles, all ready to be laid down in the midst of the structure when it requires repair. It is like the stone already hewn and shaped in the quarry for the builder; transport only, and not local labour, is wanted. The meat that is eaten is not metamorphosed until it has served its purpose, taken part in the work of the muscle, and, like every other part so doing its work, is then metamorphosed and thrown off, first from the muscular fibre into the muscular

juice, and thence into the blood and the urine. (The late Dr. Francis Sibson, p. 165.)

Cystitis.—*Exclusive Milk Diet.*—Some cases of cystitis are very much benefited by an exclusive milk diet, and these are not only cases of acute and recent cystitis, but chronic cases of one or two years' duration. For instance, a lady may complain of having to pass urine every five or ten minutes, until her life seems worn out from the continued want of rest. She has been under various modes of treatment without any benefit. Put her upon an exclusive milk diet, and although the improvement may be slow, it will be pretty certain. The improvement may be rapid—and indeed a cure may be established almost from the time of commencement of the milk diet. The *modus operandi* of the milk is sufficiently obvious. The urine is largely diluted with water, and rendered mild and unirritating by the digestible nature of the food; the bladder, therefore, being comparatively undisturbed by its contents, reverts to its normal condition, the inflammation of the mucous membrane subsides, and the morbid secretion of puriform mucus ceases. An exclusively milk diet would probably be found very suitable, for most patients during the first few days after the operation of lithotrity, the object being of course to lessen, as much as possible, the inflammation and catarrh resulting from the mechanical irritation of the mucous membrane of the bladder. I have recently seen two cases in which the vesical irritation and catarrh resulting from a stone in the bladder were much mitigated by the milk diet, the patients being thereby brought into a more favourable condition to undergo successfully, the one the operation of lithotomy, the other that of lithotrity. (Dr. George Johnson, p. 283.)

Enlarged Prostate. — *Catheterism.*—What are the circumstances which should determine us to commence habitual catheterism for a patient whose hypertrophied prostrate prevents him from emptying the bladder by his own efforts? Why should we wait for chronic cystitis?—a condition which of all others it is highly desirable to avoid in an old and already incompetent bladder, leading as such an affection naturally does to thickening of tissues and loss of extensibility on the part of the organ. Is it not, on the contrary, the very condition we desire to avoid, and do mostly avoid, by commencing the use of the catheter at a sufficiently early period? Of course I know full well that in past days, when catheterism necessarily meant the use of the large metallic instrument, often painfully passed, to say the least, chronic cystitis was an ordinary and frequent result of catheterism. But it rarely is so now, with the soft and flexible instrument of moderate

size, if only it is used at an early period in the case, and before considerable accumulation has taken place; the removal of a large quantity being mostly, I do not say invariably, followed by local and general disturbance. It happens, in the management of these cases of continued and chronic retention due to slowly advancing hypertrophy of the prostate, that the longer the use of the catheter is postponed after the early stage of the malady is passed, the worse will be the symptoms. If you find the patient affected by loss of rest—one of those things which sap the foundations of life in elderly men,—pass the catheter the last thing at night for him, and mark the result. If he obtains four or five hours of continuous sleep after the bladder has been emptied—a common occurrence—you have reason enough for persevering, and he will learn to use the instrument himself every night, and will be exceedingly grateful to you for the relief he has obtained.—(Sir Henry Thompson, p. 276.)

Filiform Bougies.—French filiform instruments measure from one millimetre in circumference upwards, while whalebone bougies may be had of a still smaller size. Some are of the same thickness throughout, but most of them are made with the shafts thicker than the points, the tips measuring one millimetre, and gradually increasing to one, two, three or more millimetres as the proximal extremity of the instrument is reached. Flexible catheters which, from their minute size, may be regarded as filiform, are also to be had. I have lately received a supply of these instruments from Krohne and Sessemann, which they tell me are the smallest catheters yet made; they measure rather more than three millimetres at their points, and four at their shafts, and contain strands of very delicate wire in place of the ordinary stylets. They are thus considerably smaller than No. 1 English. The mode of using filiform instruments hardly requires any notice, as it does not vary in any very important particulars from that followed in the case of other flexible instruments. A little difficulty is sometimes experienced in passing a filiform bougie to the strictured point, as its fine point is apt to become engaged in enlarged follicles so often present in urethræ which have been the seats of inflammations. This difficulty may be easily overcome by first filling the canal with oil. Filiform bougies with angular points are chiefly useful in assisting one in finding the orifice of a stricture when it is placed to one side instead of being in the mesial line; when the strictured point is reached the bougie should be gently twisted from side to side until the point engages itself in the opening, when it may be pushed through the

strictured portion of the canal into the bladder. Whalebone bougies with twisted or corkscrew points are useful in traversing long and tortuous strictures. The slender catheters just mentioned are warmly recommended by Sir H. Thompson, on account of their ability to transmit urine, and thus to assure the operator that the instrument has really reached the bladder. (Dr. J. C. Ogilvie Will, p. 268.)

Stone in the Bladder.—Supposing a stone to have passed from the kidney into the bladder, if the stone be small, it may be at once passed out, and the best plan to help this is that described by Sir T. Watson. Have the patient's bladder full, place him on hands and knees so as to keep the neck of the bladder low, let him for a time prevent the passage of urine by grasping the penis, and then suddenly take off the pressure, and strain forcibly to empty the bladder. If, however, the stone is unsuspected, or cannot be got rid of in the way described, it is sure to grow. You may have a stone with an uric acid nucleus, and a ring of phosphate succeeded by another ring of acid, and so on; the explanation being that the original irritation causes alkaline urine and phosphatic deposit; then a period of rest and treatment restores the normal reaction for a time. The prominent symptoms of vesical stone will be (1) *frequent micturition*. Very few people notice how often they pass urine; it is usually about six times in twenty-four hours; but, if they have to rise in the night, they notice it, for it breaks their rest. (2) *Pain*, often referred to the penis, and mostly after the bladder is emptied; (3) but the only real proof is to *feel the stone*. The sound used may be solid, or hollow, but now we always use one with this small curve: be quite sure you get into the bladder; it is possible to stick in the prostate. You will make sure by injecting water, and never forget to feel *behind* the prostate. It was a saying of Coulson's, "In every case of enlarged prostate, suspect a stone." (Mr. Christopher Heath, p. 280.)

Stricture.—Always, whenever possible, use soft in preference to solid instruments. Treat every stricture by simple dilatation if you can. If that is not successful, or if there is great difficulty in the first introduction, employ continuous dilatation followed by simple dilatation; and if the stricture refuses to yield to these means, do internal urethrotomy, using some instrument by which the incision can be regulated accurately by the hand of the surgeon. Extraordinary means, such as external urethrotomy, puncture of the bladder, &c., can only be required in rare and exceptional cases. (Sir H. Thompson, p. 267.)

Stricture Complicated with False Passages.—The presence of a false passage, or a sac left by an abscess, was always a troublesome complication of stricture, and in this country often rendered catheterism impracticable and an operation imperative, as English surgical works recorded no means by which the difficulty could be overcome, beyond the suggestion that, as false passages were usually situated in the floor of the urethra, they were best avoided by the employment of a metal catheter kept well pressed against the roof of the canal during introduction. But as false passages might be found in the roof of the urethra as well as in the floor, and an accurate diagnosis was not always possible, the suggestion was not of much value. Now, there were several resources open to the surgeon, the employment of any one of which would usually prove successful. In one instance Mr. Teevan observes that he had practised the manœuvre originated by Mercier, which was founded on the simple fact that two bodies could not occupy the same space at the same time. If a full-sized cylindrical elastic catheter were introduced into the urethra, it would either slip into the false passage or pass on into the bladder. If it went into the bladder, all was well; but, if it passed into the false passage, it could be retained there whilst a small elastic catheter was introduced. If the first instrument completely filled the false passage, the second one would necessarily go into the bladder, as there was but one road open to it to follow. (Mr. W. F. Teevan, p. 268.)

Varicocele. — *Ricord's Operation.* — Instead of employing a needle with an eye near the point, a common surgical one, with a broadish point, should be selected. After threading it with a long loop of silver wire, the wire is to be knotted at a point distant about two inches from the eye of the needle, and the wire firmly twisted from this knot to the eye. The vas deferens having been separated from the mass of veins, the needle is to be passed through the scrotal wall, between the vas and the veins, and brought out at the other side. The wire must be drawn out so far as to leave the point at which it was knotted outside the scrotum. The needle may now be removed by slitting the wire at the eye. A blunt-pointed needle or probe, armed exactly as the needle was, is then to be inserted at the aperture of exit of the former one, passed over the opposite side of the veins, and brought out at the original point of entrance of the first needle, and the wire pulled out, and the probe removed, as before. The veins now lie between two double wires, the one in front, the other behind. There are two openings in the

scrotum, through each of which there project two free ends, and a twisted portion of wire which has a loop directly in front of the point where the wire was knotted. The ends of each wire are then to be passed through the corresponding loop, and pulled tightly and forcibly in opposite directions until the vessels are completely strangulated. Compression should be kept up by fastening the ends around some resisting body, such as Mr. Tufnell suggested—viz., "a piece of watch-spring bent into an arc, the effort of which to restore itself keeps up continuous tension upon the cord." The twisted portions project on either side, and are thus available as retractors. When compression has been kept up for a sufficient length of time, the veins may be readily freed from pressure by snipping off the free ends, and making traction on the twisted ends. (Dr. J. C. Ogilvie Will, p. 287.)

The plan recommended by Dr. Will is very similar to that employed by me for many years. I employ a small trocar and canula to traverse the tissues behind and in front of the veins, and I pass a loop of strong wire through the canula, one behind and another in front of the vessels, taking care to place these loops in opposite directions. I find the trocar and canula a very speedy and easily guided means of placing the wires in the desired position. I introduce two sets or pairs of wires, the one below the other—*i.e.*, nearer the scrotum—and they are tightened, as described by Dr. Will, by passing the free ends through the loops, and when drawn tight the loops disappear within the tissues. The constriction is perfect, and has always, in my experience, performed the part required. I have never had to abandon the wires, nor have they ever failed to divide the veins. (Mr. G. H. B. Macleod, p. 292.)

AFFECTIONS OF THE BONES, JOINTS, &c.

AMPUTATIONS.—As to cutting flaps, all the methods in use have their advantages in appropriate cases. When the surgeon can do as he likes, it seems to me that the best stump in the thigh or arm is formed by separating skin-flaps on the anterior and posterior aspects of the limb, making one (usually the anterior) much longer than the other; then making a circular incision through the superficial muscles; then another down to the bone; and, finally, dividing the bone about an inch higher up. Thus the end of the bone is well covered with muscular tissue, and the scar is fairly away from pressure. In the leg and forearm, Teale's method, or some modification of it, seems to give the best results—

i.e., the bones should be covered by a good cushion of muscular tissue, and the skin-wound should be placed away from the face of the stump. It is true that the muscular tissue of the stump will probably become atrophied, and ultimately disappear, but in the meanwhile the cut ends of the bones will have become covered with new tissue, and will not adhere to the face of the stump. The objection to the old circular method is, that the scar necessarily crosses, and almost always adheres to, the ends of the bones; while the objection to all those methods in which the scar is placed out of the way of pressure (including Teale's and Carden's) is, that the length of the flaps renders some amount of gangrene probable in weakened or unhealthy persons, and must in all cases render rapid union less likely. If due provision for drainage is made the approximation of the rest of the flaps accelerates healing, and improves the shape of the stump. The best stumps, as far as I have seen, are those in which the edges unite most rapidly. As one of the essentials of success in amputation, I would specify rest of the stump. It is most important, not only for the comfort of the patient, but for his safety also, as I believe, to protect him from those spasms which indubitably disturb the parts and distress the patients, and probably aid in setting up fatal inflammatory action. To this end the gentle, equable support of a splint evenly bandaged on to the stump seems to me most useful. Acting on these general ideas, I always use sutures to support the flaps, and leave them in for a time which is practically unlimited. Even when they act, if only to a slight extent, I see no object in withdrawing them; for if they still afford moderate support to the flaps, they will avoid the use of strapping, and thus diminish the handling to which the stump is subjected. If the seeds of pyæmia are always floating about our wards in clouds, how comes it that the growth is not developed in one wound out of a hundred? (Mr. T. Holmes, p. 194.)

New Method of Amputating at the Hip-Joint.—Mr. David Newman, of Glasgow, proposes a new method of amputating at the hip-joint. For details see the paper at p. 173. To arrest hemorrhage, the pressure is neither applied to the aorta, nor to the femoral as it passes over the brim of the pelvis, but is brought to bear upon the base of the flaps.

Bloodless Operations.—The recently introduced bandage of Esmarch is a most convenient apparatus for dissecting operations in the limbs, and is, I think, an improvement in many cases of amputation. You saw me amputate a child's thigh, two or three weeks ago, after this "bloodless" method, and you could see how well in that instance it deserved its name

—for hardly a spoonful of blood was shed. It is true that this is not always the case. The vessels will ooze for a time after the removal of the bandage; and this oozing sometimes causes about as much loss of blood as would have occurred with the tourniquet. The main utility of Esmarch's bandage is in cases of excision, of operations on bones, and of removal of tumours, particularly when deep-seated—cases in which it may be of the most vital importance that the surgeon may be able to see all the tissues as clearly in the living as in the dead subject. (Mr. T. Holmes, p. 195.)

Compound Fractures.—*Compresses soaked in Tinct. Benzoin Co.*—Compound fracture of a limb may be treated with almost uniform success by closing it at once with compresses of lint soaked in the compound tincture of benzoin. The fracture must first be reduced, and the wound then closed with several pieces of lint soaked in the tincture. The limb should then (supposing it to be the leg) be fixed upon a posterior splint with two lateral ones and swung in a cradle. The lint need not be removed for from twenty-five to twenty-eight days, when the wound will be found to have healed, and the fracture to have become consolidated. A flannel gum-and-chalk-bandage may then be put on, and the patient allowed to get up. (Mr. Thomas Bryant, p. 187.)

Excision of the Elbow-Joint.—In excision of the elbow, no transverse cut across the triceps should be made, and it is essential to securing extension of the foreaam, which power is not unfrequently lost, to preserve those tendinous fibres of the triceps muscle which are sent from beyond the attachment to the olecranon to blend with the fascia of the forearm, and especially with that portion of the fascia overlaying the anconeus muscle. Commence the operation by a longitudinal incision at the back of the limb, in length three or four fingers' breadth both above and below, crossing the point of the olecranon. Next sink the knife deep into the triceps muscle, and divide it also longitudinally into two portions, the inner one of which is the more firmly attached to the ulna, while the outer portion is continuous with the anconeus muscle, and sends some tendinous fibres to blend with the fascia of the forearm. It is these latter fibres that are to be scrupulously preserved. (Surgeon-Major J. H. Porter, p. 181.)

Excision of Hip-Joint.—Use Mr. De Morgan's splint. This may be described as a long Liston's thigh-splint, having at its foot-end a cross-bar, in which is a longitudinal slit for a pulley, which can be shifted in it to any required distance. There is another pulley where the short and long pieces of the splint join. The splint is applied to the sound limb, and

counter-extension is obtained by a perineal band on this side, whose extension of the operated limb is effected with a cord fastened to a stirrup of plaster, or better to a loop of bandage stitched to the sides of the leg of an ordinary stocking, which clings sufficiently to the limb when secured by a few circular turns of roller. The cord running over the pulleys passes up the outside of the long piece of the splint, and is kept tense by an India-rubber accelerator attached to the upper or trunk end of this. A little perforated slip of wood such as is used for tightening tent-ropes allows easy regulation of the tension. (Mr. J. W. Hulke, p. 193.)

Excision of the Knee-Joint.—*New Mode of Performing the Operation.*—Make a semilunar incision about three inches in length on each side of the joint, the lowest point of each incision being thoroughly dependent for the exit of pus or serum. Divide the lateral ligaments on each side, and reflect the tissues till the synovial cavity in front is well opened. If there be adhesions in front, they are divided. A wide director is passed behind the joint in front of the posterior ligament, and with a narrow bistoury the crucial ligaments and any adhesions there may be between the bones are divided. A metal retractor is inserted in front of the bones to secure from injury the tissues in front and the skin and tissues loosened from the sides, whilst the bones are being sawn. The blade of a Butcher's saw is passed behind the joint; and, this being connected with its frame, a thin slice is sawn from the joint-ends of each bone. The sawn surface of this slice is the exact counterpart of the surface left behind; and if, on examination, it appear to be healthy, I pass on to the patella, which is left if healthy, or sliced if its cartilage be ulcerated, I formerly used a chain-saw for the sections, but have relinquished it in favour of Butcher's. (Mr. W. Knight Treves, p. 180.)

Excision of the Joint Between the Os Calcis and Astragalus.—Excision of such a joint as that between the os calcis and astragalus may appear an excess of conservatism in surgery, but the employment of the bloodless method of operating enables us to ascertain the exact extent of disease in a way which could not formerly be done. Moreover, the antiseptic treatment gives advantages which must conduce to success. Having carefully studied the anatomical relations of the joint under consideration, I found that its entire extent could be best exposed without injury to surrounding structures in the following way: The foot having been placed in the extended position, and resting on its inner aspect, an incision, commencing about an inch above the tip of the

external malleolus, and carried along its posterior border in a curved direction to the calcaneo-cuboid joint, thoroughly exposes the posterior and external portion of the joint, when the peroneal tendons have been drawn outwards, and some ligamentous bands divided. This incision will be found to run along the inner border of the tendon of the peroneus brevis muscle. The anterior and internal portion of the joint can then be exposed by placing the foot still in the extended position on its outer aspect, and making an incision from the tip of the internal malleolus along the course of the tendon of the tibialis posticus, as far as the prominence of the scaphoid bone, drawing forwards this tendon, and carefully drawing backwards the other tendons and the posterior tibial vessels and nerve. By making the first incisions through the skin and cellular tissue only, and so ascertaining the exact position of the tendons likely to be injured, then cutting down through the periosteum to the bone, and with a periosteal scraper separating to a sufficient extent the periosteum, together with all the other superficial tissues, there is little risk of injuring any of the tendons or other important structures. Both aspects of the joint having in this way been exposed, it will be found that by means of the chisel and mallet the articular surfaces can be easily and accurately removed, the posterior portion being removed through the external incision, and the anterior portion through the internal one. Should there be any disease in the hollow or fossa between the two articular surfaces, it can be readily reached and removed with the chisel or gouge through either of the incisions. (Mr. Thomas Annandale, p. 185.)

Genu Valgum.—In a case of genu valgum it was determined to make subcutaneously an oblique division of the elongated inner condyle. The following operation was decided upon, and put into practice on the left knee:—The patient was chloroformed, the knee flexed as far as possible, and the thigh turned outwards. A long and strong tenotomy knife (Adams's) was introduced through the skin, three and a-half inches above the tip of the internal condyle on the inner side of the thigh, and so far back as to be opposite the ridge of bone running between the linea aspera and the condyle. Its blade was carried forwards, downwards, and outwards, over the front of the femur, with its cutting edge directed to the bone. When its point could be felt under the skin, in the groove between the condyles where the patella would normally have been lying in the flexed position of the limb, the cutting edge was pressed against the bone, and the soft parts and periosteum divided by one slow firm movement in with-

drawing the knife. The external wound thus made was about one-third of an inch long, and formed the entrance to a subcutaneous tunnel, running obliquely over the front of the femur, and ending in the cavity of the joint. Adams's saw for subcutaneous division of the neck of the femur was introduced into the tunnel, and the condyle sawn off by directing the edge of the saw straight backwards. The position of the saw could be exactly controlled by feeling its point working gradually backwards in the groove between the condyles. As soon as it was estimated that the condyle was almost entirely separated, and that the saw had arrived near the popliteal space, the saw was withdrawn. The knee was completely extended, and then with the hands, and the operator's knee as a fulcrum, the patient's knee was forcibly straightened by bending the leg inwards. The remaining connexions of the condyle with the femur gave way with a crack on the application of very moderate force, and instantly the leg became as straight as a healthy limb, and could even be put into a somewhat bandy (genu varum) position. *The whole of the operation was conducted under carbolic spray, with a minute observance of Lister's antiseptic precautions, and the limb was put up in antiseptic dressings.* It was then simply bandaged to a Liston's long splint, the thigh and leg being kept in the same line by pads, and the boy was placed upon a hard mattress. (Dr. Alexander Ogston, p. 210.)

Surgical Ligatures.—*Carbolised Ligatures* versus *Torsion*.—Of the vexed question of ligature or torsion. We have ceased at this hospital to use acupressure, because, after a satisfactory trial, we thought it decidedly inferior to the old plan of the silk ligature, and some of my colleagues still use the latter. I have, as a general rule, abandoned it for the carbolised catgut ligatures, which I have found equally efficient against primary and secondary hemorrhage, and which offer (I am again speaking only generally) no obstacle to the rapid union of the wound. You saw this illustrated in the case of the little child whose thigh was amputated. The wound was healed almost entirely, and the patient out of bed, on the fourteenth day after the amputation, no trace of the ligatures having been seen. I prefer this plan to torsion (though quite admitting the efficacy and safety of torsion in skilful hands), inasmuch as it is very much easier to apply, and involves much less handling and contusion of the parts. In an amputation in which torsion is used, though the larger and more obvious vessels may be easily and quickly secured, we often see much difficulty in dealing with those which come less distinctly into view; and this difficulty is greater if the

gun cotton, a little tincture of benzoin being finally admixed.) In removing the styptic colloid dressing common water should be scrupulously avoided, and a mixture of alcohol and ether employed, or equal parts of absolute alcohol and distilled water, warmed to a little above the heat of the body. It has been noted that the dressing was not touched for five days after the operation. Once divided parts—be they hard or soft, bones or muscles, skin or nerves—are adjusted with a view to union, the less they are disturbed the better. Metallic sutures so very rarely cause any irritation, that they may be inserted very near each other with impunity. Sutures far apart, with gaping intervals, are comparatively useless. If the cut surfaces are to adhere they must be brought into contact and kept there, and for this purpose metallic sutures half an inch apart, or even less, are most efficacious. I often apply intervening strips of lint soaked in styptic colloid, but in this case only place over the wound a layer of fine cotton wool, and a pledget of picked oakum. The best cotton wool for surgical dressings is that sold for jewellers in thin sheets, about eighteen inches by twelve, with alternate layers of tissue-paper. (It is supplied by S. Jevons, Worcester-street, Birmingham, at 2s. 9d. the lb. parcel.) (Mr. Sampson Gamgee, p. 206.)

New Antiseptic and Astringent Lotion. — Salicylate of Iron.—Salicylic acid and its compounds are excellent antiseptic applications to wounds after operations, but like carbolic acid they are not astringent, and therefore allow free capillary bleeding. A solution of salicylate of iron is both astringent and antiseptic. (Dr. Robert Kirk, p. 365.)

AFFECTIONS OF THE SKIN.

Abscesses.—*Hyperdistension with Carbolised Water.*—Carbolic acid acts directly upon the interior of an abscess as, what might be called, an antipyogenetic—not simply checking or removing putrefaction and its results, but arresting the formation of pus and setting up a healthy action. It is an excellent plan after opening an abscess to wash it out, and even hyperdistend it by a watery solution of carbolic acid. The hyperdistension ensures its application to the interior of the several septa or chambers of which some abscesses consist. The operation may be performed whilst the patient is under the influence of ether, or the integuments may be frozen by the ether-spray. The following are required: A scalpel where an incision is needed, no open sinus existing; carbolic acid lotion (one part in twenty) diluted to one in thirty by the addition of warm water before using it; a per-

forated elastic drainage-tube; carbolised oil (one in twelve) on lint for dressing the wound, and gutta-percha tissue for covering this; some ordinary adhesive plaster; some tenax to receive any subsequent discharge (which, however, is very slight); an ordinary two or four-ounce syringe. When it is desirable to make continuous pressure over an abscess after opening it, a pad shaped to the needs of the case, and, filled with shot, will be found useful. It acts more effectually than a sand-bag, and is easily made and adapted. The operation is begun by cutting into the abscess (if no sinus exist), the opening made being of sufficient size to admit one of the fingers. The pus is then allowed to escape, the abscess being emptied as completely as possible. The nozzle of a syringe is next passed through the opening, and the skin is drawn closely around it by the operator with his left hand; the contents of the syringe are then passed into the abscess-sac. (Mr. George W. Callender, p. 217.)

There can be no doubt but that carbolic acid checks suppuration—producing a clean healthy surface—in fact it arrests the death of granulation cells. Its use in the treatment of large abscesses—by hyperdistension with it, diluted with water as introduced by Mr. Callender—is increasing in favour. In a case in the Manchester Infirmary, the largest abscess, that on the inner aspect of the left thigh, was opened with a scalpel; and carbolic acid lotion, 1 in 20, mixed with half its bulk of warm water, was injected, by means of a Higginson's syringe, continuously into the cavity until it was fully distended, and the fluid returned almost clear. A drainage-tube of perforated India-rubber was then introduced, and the abscess was dressed with "cere-cloth" soaked in glycerine and carbolic acid; and covered with oil-silk. Each day these dressings were renewed, and twice a little carbolic lotion was injected. After the tapping on the 19th, when nearly eighteen ounces of pus were evacuated, the discharge lost its purulent character, became very scanty, and almost entirely serous in nature. (Mr. F. Ashton Heath, p. 219.)

Erysipelas.—Having during a course of several years, in hospital and private practice, used a variety of local applications in simple or cutaneous erysipelas, I have for two years discarded all for the perchloride of iron, which I have never seen to fail. The form in which I use it is the following: Equal parts of liquor ferri perchloridi fortior (B.P.) and spiritus vini rectificatus; the whole affected surface, and about an inch beyond the affected parts, to be painted over with the lotion by means of a camel's-hair brush. (Dr. W. Leavens White, Brit. Med. Journal, Dec. 9, p. 750.)

Scrofulous Lymphatic Glands.—At first don't interfere much except by constitutional treatment. But the first stage may subside, and the result may be a hard gland, lympho-sarcoma. Though caustics may be employed for the removal of these gland-masses, yet, especially where they are freely movable or encapsuled, excision is the best treatment. The characteristics of the third degree of the inflammatory state differ from those that precede it. The glands have now mostly ceased to enlarge, unless they are doing so simply as the result of the irritation occasioned by the more active change going on in their neighbourhood; they are no longer free, but form a mass in which the outline of the original glands may perhaps still be distinguished; but whether this be the case or not, the whole is bound to the overlying skin and to the surrounding tissues by inflammatory adhesions. Hard in some parts, it is red, tender, and softening in others; and here fluctuation may be detected; and then, if the pus be evacuated and poultices applied, the opening, in lieu of contracting and healing after discharging a slough, often remains patent, and a granulating mass protrudes, which may exist, with little or no tendency to heal, for a long time. The treatment in this stage is not that by excision. The patients are generally sickly, and bad subjects for operative interference, even were it possible; but it is scarcely so in many cases, for the glands, now matted by inflammation, often enclose between them important structures. Where, however, it is feasible, the measure usually adopted is to incise the overlying tissues and then scoop out all that is removable with a blunt scalpel or spoon-handle. In by far the greater number of patients the only application is a poultice, or simple water-dressing, time being invoked to complete the cure. Another alternative yet remains in the removal of the glands by caustics; and I shall only mention that by chloride of zinc, for the electrolytic caustic depends upon the formation of this salt for its destructive properties. The whole treatment depends upon the production of the chloride of zinc, in the tissues of the body, at the expense of the chlorides in the serum, which are caused to unite with a slip of metallic zinc thrust in at the spot destined to be destroyed. To understand this, I need but recall what happens in a Smee's battery when the electrodes are united and the circuit consequently completed: not only is an electric current established, but the acid uniting with the zinc forms sulphate of zinc, while the hydrogen is evolved at the electro-negative or silver plate. If in lieu of sulphuric acid we employ chloride of sodium, a parallel change takes place: chloride of zinc is formed, and the sodium is set free at the silver plate, and becomes at once

oxidised into soda. In this case, therefore, we have, instead of the two inert metals, two active salts; and if any albuminous substance, capable of being acted upon by the chloride of zinc, be present in the solution, it will be simultaneously affected. Let the silver and zinc plates be therefore placed one on either side of a piece of flesh, and be connected together by a copper wire, and the same changes will occur. The flesh is—for our purpose—a firm albuminous basis, bathed in serum or chloride of sodium solution; and the chloride of zinc that is formed, having a powerfully coagulating (and therefore destructive) effect upon albuminoid tissues, will cause marked changes in the space of a few hours. The flesh under the zinc plate will be no longer red, soft, and alkaline, but will be converted into a grey friable material, strongly acid to test-paper; while the presence of the soda under the silver plate may be proved by its strongly alkaline reaction. Each molecule of chloride of zinc, as soon as it is formed, or even in the very act of forming, exerts an influence upon the albumen with which it is in contact, and coagulates it; and it is to this action of the chloride of zinc in a "nascent state"—to use the chemist's phrase,—or, in other words, to its action by successive particles at the moment of their formation, that the absence of pain, when the living body is being operated upon, must be ascribed. (Dr. C. H. Golding Bird, p. 309.)

Skin Affections.—*Glycerole of Nitrate of Bismuth.*—Nitrate of bismuth, which is so utterly insoluble in water, is perfectly soluble in glycerine; and the presence of glycerine in water is capable of delaying, more or less, the precipitation of the nitrate of bismuth in it. So that where glycerine is a permissible vehicle we have now a ready means of *applying* a simple solution of bismuth; and where glycerine is not permissible as a vehicle we have equally a ready means of *administering* a simple solution of bismuth in water, provided we do not mix it too long before it is required to be used. As an application to the skin, or to the vagina or uterus, or to the throat, when bismuth in solution is required, nothing can be better than the solution of the nitrate of bismuth in glycerine. As a mixture to be taken internally, nothing more is necessary than that the mixture should be ordered as a solution of the salt in glycerine—one teaspoonful for a dose, to be mixed in half a wineglassful of water before taking it. Then, if the patient does not look too long at it before he swallows it, he will certainly swallow a perfectly clear solution, and one, moreover, which will probably do him more good than any other preparation of bismuth that has yet been

devised. As an application for diseases of the skin a field is opened out for the employment of bismuth with an amount of efficiency and precision that has never yet been possible; and the variety of diseases of the skin in which even the comparatively inert, because insoluble, trisnitrate has been found of service are too numerous to refer to in this place. In diseases of the female genital organs, accompanied with tenderness and profuse discharge (a sufficiently common class of cases), we have here a remedy which requires only to be carefully tested to insure, most probably, very excellent results. In inflamed sore throats its value as a paint or as a gargle will most likely be very considerable; and as an injection in gonorrhœa, after the acute stage has passed over, or in cases of chronic gleet, attended with an irritable state of the urethra, it cannot but prove of signal service. (Dr. Balmanno Squire, p. 306.)

SYPHILITIC AFFECTIONS.

Vaccino-Syphilis.—It seems clear that cow-pox and syphilis can be conveyed simultaneously by inoculation. When this takes place, the two diseases are developed independently and without interfering the one with other, and there is a singular uniformity in the course which they are found to follow. The vaccine vesicles are developed as usual, but at the end of a month or five weeks, and usually not till after they have healed, a chancrous induration appears at one or more of the inoculated points, and a few weeks later the ordinary train of secondary affections follows. The prevalent opinion that it is the blood, and not the vaccine lymph, which conveys the contagion — a view which seems to be strengthened by the fact that Rollet and others have vaccinated with lymph from syphilitic children, carefully avoiding any admixture of blood, and the result, as regards syphilis, has hitherto been negative. Mr. Hutchinson's cases are all examples of direct communication from a diseased child to those vaccinated from it, and this is a mode of transfer which one's mind readily admits; but it is very difficult to conceive that healthy persons so inoculated, when made use of a week afterwards to vaccinate others, and long before they have themselves developed any syphilitic symptoms, should be equally capable of conveying that disease, as is stated to have happened in most of the continental groups of cases. Can it be possible that a syphilitic inoculation which does not affect the system sufficiently to produce even a local result in the form of a primary sore for five or six weeks, and no general result till some weeks later, should in one week have so contaminated the whole

mass of the blood as to render it, or its products, inoculable at the end of that short time? This would imply a general and thorough infection of the system in the early part of the incubation period—a view in the pathology of syphilis which, as far as I know, has never yet been suggested. It is so extremely unlikely that, for myself, I cannot accept the facts as authentic without much fuller and more rigorous investigation. If it were true the prospect would be indeed alarming. (Mr. J. R. Lane, p. 302.)

AFFECTIONS OF THE EYE AND EAR.

Artificial Drum-Heads.—Mr. Yearsley's artificial tympanum was a pellet or plug of cotton-wool, but at the present day, many English aurists who prescribe cotton-wool in cases of perforated membrane, direct the patient to form it into a disc. Of my own knowledge Mr. Yearsley so directed his patients. Dr. Allen, who was Yearsley's successor, did the same. The difference between various artificial drum-heads is not one of "disc" or "pellet," but of material. It is certain that the firmer the wool is applied the better the support. (Mr. Lennox Browne, Brit. Med. Journal, Nov. 4, p. 587.)

Ear Speculum.—Mr. Millikin, of St. Thomas's-street, Borough, has brought out an ingenious and yet simple self-retaining ear speculum, a description and diagram of which will be found at page 301.

Eye Douche.—Messrs. Arnold & Sons have made for Mr. Priestley Smith, of Birmingham, a eye-douche on the principle of their "simple enema syringe," an engraving and description of which is given at page 300.

Squint.—I pinch up with forceps an ample fold of conjunctiva and sub-conjunctival tissue immediately over the insertion of the internal rectus tendon, as in von Graefe's operation; divide this and separate the conjunctiva pretty freely from the subjacent parts, so as to avoid shrinking of the caruncle (as suggested by M. Liebreich); pass a small hook along the under border of the tendon, and cause its extremity to project beyond the upper border. I then cut upon the point of the hook through the stretched conjunctiva with scissors, thus making a counter-puncture, as is Mr. Bowman's practice, and divide the remains of the tendon under the strip of conjunctiva which is allowed to remain. Shall we operate upon one or both eyes? We have seen that the eye which was closed, whether right or left, became for the nonce the squinting one. Most authors recommend us to do one eye only at a time,

and this would be, no doubt, a safe procedure if we could do as we pleased with our patients; but, in point of fact, we cannot do as we please, and in deciding upon the course to adopt we must take into account various outside factors which are likely otherwise to disturb the result of our calculations. Two or three operations involve the repeated administration of anæsthetics, which patients dread more and more each time they are used. If I operate upon one eye only, the other eye is certain to squint, just as it does if closed or covered with ground glass; and, indeed, the operated eye itself will often in such cases look as if it wanted something more doing at it. When this is the case the parents are disappointed, while friends and acquaintances who deprecated the operation are triumphant, and often succeed in persuading them to decline any further interference. In my own practice I generally divide both tendons, and am not satisfied until parallelism is pretty nearly restored. (Dr. C. Bell Taylor, p. 292.)

MIDWIFERY, ETC.

Braithwaite's Forceps.—These are a light and small forceps, not intended for difficult cases, but for frequent use in tedious cases of labour, either from slight impaction or exhaustion of the patient's strength. They can be carried in the breast pocket and can be applied so easily as hardly to attract the patient's attention if she were not told. The peculiarity of the instrument consists in both blades being introduced at once directly into the hollow of the sacrum. The blades and handles are accurately fitted to each other, the convexity of the inner blade to the concavity of the outer one, and this adaptation is maintained by a metallic clip on the handles. They are thus practically one blade during introduction, and before separation by removal of the clip. There is nothing peculiar in the blades when separated. If a pair of Simpson's short forceps is arranged with one blade inside the other, a very good idea of the instrument will be obtained. Although, however, I made Simpson's short forceps the model of mine, the blades of the latter are rather lighter and narrower, to lessen the weight and facilitate introduction. (The total weight with the clip is about 10½ ounces.) The introduction of the blades into the sacral hollow is not felt, because the external parts of the mother are not touched by them. Being small and narrow they are passed between the first two fingers of the left hand and the fœtal head. The clip is to be loosened a little before introduction, and after introduction is to be removed altogether; the blades are then to be glided round the head, in opposite directions and locked in the

ordinary way. I generally do this as each pain comes on, not during the complete absence of pain, nor during the intensity of one. When the blades are locked, the extremity of the upper blade, in the first or common position of the head, will lie on the right temporal ridge, just within the line of the hairy scalp; and the lower blade will lie behind the left ear; in the second position this will be reversed. (Dr. James Braithwaite, p. 313.)

Use of the Forceps in Delivery.—Dr. Jurney, Professor of Obstetrics in Ohio, believes the ordinary method of using the obstetrical forceps a very frequent cause of laceration of the perineum. He makes little or no traction during the pain; but, in the interval of the pains, he uses traction enough to prevent recession of the head of the child, holding it firmly against the perineum. This prevents a re-contraction of the perineum, and converts a violent intermittent distensive force into one which is slowly acting and persistent. Under this moderate but constant pressure, the perineum surely and safely dilates, and laceration is avoided. The reversed rule, therefore, in the use of the forceps will stand thus—make no traction during the pains; let the traction be made in the interval of the pains. (Dublin Journal of Medical Science, January, p. 96.)

Carcinoma-Uteri.—*Ergot.*—In cases of medullary cancer of the uterus, and also of advanced epithelioma in the same region, I have been struck with the marked relief often derived from the administration of ergot, in doses of thirty minims every six hours. There is a relief from the intense throbbing, which, as a rule, only subsides with each attack of hemorrhage, which of course brings with it great exhaustion. I consider the ergot acts in the ordinary way, by lessening the amount of blood in the uterus; and it may also check, to a slight extent, the rapid breaking-down of the affected part. (Dr. A. E. Aust-Lawrence, p. 348.)

Ovariotomy.—*New Treatment of the Pedicle.*—Kovacs has in two successful cases of ovariotomy used the following method of tying the pedicle. After having made the section of the pedicle with scissors, he divided it into four parts and tied each artery separately with catgut. The hemorrhage being thus completely mastered, he turned back and sutured the peritoneum around the stump. He left a drainage-tube in the lower angle of the wound. (British Medical Journal, Dec. 9, p. 755.)

Post-partum Hemorrhage. — In post-partum hemorrhage a drachm of laudanum will rally the exhausted strength,

restore consciousness, rouse the uterus to action, and arrest the bleeding when six times ten minims in water would allow the patient to bleed to death. In syncope a tablespoonful of undiluted brandy will have an effect which twice the quantity drowned in water would fail to produce. We defeat a paroxysm of ague and cut short the disease, or suppress neuralgia, by a single large dose of quinine, when repeated doses of average amount have entirely failed. (Dr. W. H. Broadbent, Practitioner, Feb., p. 87.)

Ether Spray.—Try the effect of the ether-spray. In one case I directed a large spray over the abdominal walls, along the spine, and over the genitals; the uterus at once responded, and the cessation of the hemorrhage was almost immediate. In a second case I lost no time in adopting a similar treatment, and with an equally successful result. (Dr. W. Handsel-Griffiths, Practitioner, March, p. 176.)

Puerperal Fever.—We are agreed as to the necessity of the midwifery practitioner avoiding all post-mortem examinations. His hands should avoid all contact with specific poisons or septic materials; and if perchance his fingers have touched anything suspicious, he should at once carefully and thoroughly disinfect them. The necropsies which seem most baneful are those of bodies which have recently died of erysipelas, peritonitis, zymotic disease, or any other inflammatory and febrile affections; and in such instances the accoucheur should not even be present in the room when the dissection is made, as, though he decline to touch, yet his person and clothes may become infected by the poison. There may possibly be a doubt whether one who dissects bodies altered by decomposition or antiseptic injections, as they are commonly found in the dissecting room of medical schools, is so liable to contaminate a puerperal patient as he who makes a necropsy soon after death. Semmelweiss, however, believed that animal matter in a state of putrefaction would cause puerperal fever. The discharges from cancer may produce fever in a lying-in woman; and this is probably true of other putrescent discharges. (Dr. W. O. Priestley, p. 340.)

Local Antiseptic Treatment.—Dr. Fritsch, of Halle, recommends the systematic adoption of certain antiseptic measures, not only with a view to prevent infection by the hand of the accoucheur or midwife, but also to remove and destroy any decomposing secretions which may develope in the uterus itself. In the first place, before any examination is made, the hands are to be well washed with soap and then scrubbed with a nailbrush and a solution of carbolic acid. The most convenient arrangement for preparing this solution is to have

a number of thirty-gramme bottles filled with concentrated carbolic acid dissolved in glycerine (thirty grammes acid to three glycerine), and to take one or two in the pocket when called to a midwifery case. By measuring out a litre of water into a basin, and adding the contents of one bottle, a solution of a proper strength is at once ready for use. Any instruments that have to be applied must also be previously disinfected. Instead of ordinary oil, carbolic oil must alone be employed to grease the hands and instruments. (Dr. H. Fritsch, p. 337.)

Secale Cornutum. — *Therapeutic Action.* — The well-known energetic influence of the ergot of rye upon the parturient uterus naturally led to the employment of such a potent drug in various other conditions of the system. Its virtue must be general and not local, and will in like measure be equally developed in whatever quarter of the body organic muscular fibre is plentifully distributed, and that its effects should largely extend to the heart, to the stomach and intestines, to the bladder, to the uterus, to the spleen and other viscera whose anatomical structure to a considerable extent partakes of this substance. Accordingly, in a heart case which lately presented itself, in which the prominent symptoms were palpitation, with some degree of valvular murmur and increased dulness over the cardiac region—in short, the ordinary signs of simple dilatation of that organ—liquor secalis cornuti was administered in 1 drachm doses three times a day, with marked benefit to the patient, and the gradual amelioration of all the symptoms. The success of such a case, of course, depended upon the absence of structural changes in the valves themselves, the pathological condition being want of tone in the cardiac muscular apparatus, or flaccid heart as it might be designated, valvular incompetency being caused by dilatation of the orifices occurring synchronously with enlargement of the organ, and not to changes in the size or capability of the valves, which resumed their natural functions when the organ and its orifices returned to their normal dimensions. (Dr. James Cuthill, p. 321.)

Subacute Vaginitis.—The ordinary magnesia mixture and lead lotion have generally been tried with but little benefit. Dr. Edis recommends swabbing the vagina freely with strong carbolic acid. A speculum is inserted, and the fundus vaginæ first touched; the speculum being gradually withdrawn, the acid is allowed to come into contact with the whole length of the vagina, stopping short just before the junction of the mucous membrane with the vulval outlet, otherwise intense burning pain will be produced. Care must

be taken not to allow any excess of the acid to run down externally. It is well to insert a pledget of cotton-wool soaked in oil just within the orifice of the vagina to prevent this; but should much burning pain ensue, the patient is directed to squeeze a little olive-oil into the passage, and to insert a morphia suppository *per anum*. In several instances, lately, this method of treatment had been adopted with marked success; two or three applications, at most, arresting the discharge, after several weeks' ineffectual treatment with ordinary lotions. (Dr. Edis, British Med. Journal, Jan. 27, p. 104.)

Uterine Fibroids.—*Use of the Actual Cautery in Enucleation of.*—What is to be done in a case of interstitial fibroid, with a closed os and cervix, which are also carried almost out of reach by the growth of the tumour downwards into the pelvis? In these cases a large mass fills the pelvis, and the vaginal canal is extremely elongated. In such a case, let the surgeon penetrate the uterine walls with an olive-shaped actual cautery. The opening so made will gradually enlarge spontaneously almost like a natural os uteri. The finger can be passed within it and the tumour partially enucleated. The process of spontaneous enucleation will gradually progress until the tumour is expelled entire. Assistance may be given by the finger, and portions which become sloughy must be removed. This curious and interesting process was accidentally discovered owing to the use of an actual cautery to arrest hemorrhage, following the introduction of an aspirator needle. (Dr. Robt. Greenhalgh, Medico-Chirurgical Transactions, vol lix, p. 41.)

Leeches.—*To make them bite.*—Dr. Correa de Serra recommends the part to be first well washed with pure water, dried, and then rubbed gently so as to warm the skin. Whatever be the number of leeches, take a small glass, and lay across its mouth a moderate-sized piece of linen. Pushing in the centre of this to the bottom of the glass, a cavity is formed, into which the leeches are thrown. The glass having been applied to the desired spot, and lightly pressed down by the left hand, tighten with the right hand the linen by drawing it round the circumference of the glass. The leeches are thus necessarily brought into contact with the skin and cannot wander about. Presently the leeches will bite, as testified by the patient, when the glass may be removed, *leaving the linen* for a little longer. (Dublin Journal of Medical Science, March, p. 246.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—TREATMENT OF TYPHUS AND TYPHOID FEVERS BY CUCA.

By Dr. SAMUEL MCBEAN, Lecturer on Botany, Durham College of Medicine, Newcastle.

Since Dr. Ringer made the important discovery that the amount of urea excreted by the kidneys stands in a certain relationship to the temperature of the body, it has been ascertained that in all febrile diseases, with the exception of yellow fever, the amount of urea formed is abnormally increased; and representing, as urea does, the wear and tear of the frame (tissue metamorphosis), it may be regarded in fever as affording not only an index to the severity of the disease, but also a direct clue to an exceedingly important line of treatment.

I will further illustrate the position I assume in this paper by submitting that, as throughout the duration of a case, *i.e.*, of typhus fever, there is present a specific morbific agency which so influences the sympathetic nervous system as to produce or permit an extent of tissue metamorphosis (indicated by the increased amount of urea excreted) not only incompatible with health, but most probably productive of the various febrile phenomena, the sum of which constitutes this disease; therefore, to restrain such metamorphosis until the morbific agency has ceased to exist, is clearly indicated as the most desirable end to accomplish in treatment. To accomplish this end, I believe we possess a very efficient remedy in cuca, the dried leaves of the erythroxylon cuca. I need not say anything of the properties ascribed to this drug (the subject having already been very freely ventilated), beyond stating that, when indulged in by a healthy person, it prevents for a time the feeling of fatigue, and diminishes the amount of urea excreted—subjective and objective signs of tissue-metamorphosis—and that in this property lies its value as a therapeutic agent.

Case 1.—Mr. R., aged 34, of a phthisical family, came under my care on September 8th, suffering from the usual premonitory symptoms of specific fever. He had been unwell for some time previously, but not confined to bed. Until September 15th, there was no perceptible increase of temperature, but from that date the ordinary symptoms of typhoid fever rapidly developed;

and on the 19th the patient's condition was as follows. He complained of severe occipital headache and vertigo; sore-throat; pain in the back at the lower angle of the scapula; slight ileo-cæcal pain; painful confusion of thought, with occasional delirium. I found the mucous membrane of the fauces much swollen, red, and ulcerated; the tongue moist and thickly coated; slight hypostatic pneumonia of the lower lobe of the right lung; sputa muco-purulent, streaked with blood; the abdomen slightly tympanitic; diarrhœa feculent. He had occasional paroxysms of profuse diaphoresis; also an abundant crop of miliaria crystallina. Decubitus was dorsal. Pulse 108. Temperature at 10 a.m., 102.40; at 10 p.m., 104. Respirations per minutes 36. The amount of urea excreted during the preceding twenty-four hours was 59.05 *grammes* (2.08 ounces). Until to-day, the treatment had been purely expectant. The patient was ordered a drachm of tincture of cuca in water every four hours.

September 20th. He had spent a better night; said he had "greater control of his thoughts." There was no delirium. Temperature at 10 a.m., 100; at 10 p.m., 102.40. Pulse 104. Respirations 30. The amount of urea excreted during the preceding twenty-four hours was 53 *grammes* (1.87 ounces); the diarrhœa was much the same; and there was still much diaphoresis. He complained that the medicine produced flatulence. The tincture was continued in doses of a drachm and a half with fifteen minims of essence of ginger, every four hours.

September 21st. There was now no confusion of thought. Temperature at 10 a.m., 99.40; at 10 p.m., 100.50. Pulse 98. Respirations 26. Urea excreted during the preceding twenty-four hours was 48 *grammes* (1.7 ounces). Much less diaphoresis and flatulence. The patient's general appearance much improved. The cuca to be continued as before.

September 22nd. No pneumonic crepitation; sputa mucous. Yesterday and to-day, he had been able to read the newspapers. Diarrhœa still continued. Temperature at 10 a.m., 99; at 10 p.m., 100. Pulse 87. Respirations 22. To-day, there were a few spots of typhoid roseola on the abdomen. He was ordered two drachms of the tincture of cuca, combined with a drachm of liquid extract of bael, with a view to restraining the diarrhœa.

From September 23rd to September 29th, the patient continued to improve; the temperature, pulse, and respiration on the 29th being about normal. The diarrhœa ceased on the 27th, and on the 29th the amount of urea excreted daily had fallen to 27 *grammes* (416.7 grains), and I now considered convalescence fairly established; but on the evening of the 30th the

patient, having become rather careless, allowed himself to be exposed to a draught of cold air from a window. This was succeeded the same night by rigors. On October 3rd, he was suffering from a severe attack of lobular pneumonia; and on the 10th, I regret to say, he died.

I have treated two or three other cases of typhoid fever with cuca, and in each case obtained a favourable result; but have selected this case for publication, as in the others I made no quantitative analysis of urea.

Case 2.—C. B., aged 46, wss seized on October 3rd with the usual premonitory symptoms of fever, and on October 10th the patient's condition was that of a well marked case of typhus fever. Temperature 104; pulse 108; respirations per minute, 34. The amount of urea excreted during the preceding twenty-four hours was 58 *grammes* (2.05 ounces). I prescribed a drachm of tincture of cuca every four hours. From October 10th until October 18th, the tincture was continued, and the dose gradually increased to three drachms every four hours. On the 18th, the amount of urea excreted during twenty-four hours was 28.05 *grammes* (nearly one ounce). With the diminished amount of urea, there was a corresponding diminution of temperature, pulse, and respiration, all of which at this date had become quite normal. In this case, no remedy save the tincture of cuca was used; and, as in the preceding case, from the time the patient began to take the tincture until convalescence was established, there was a daily amelioration of symptoms.

Case 3.—I would here refer to a case of protracted convalescence after typhoid fever. On September 17th, I was called to see Mrs. S. She informed me that she had been suffering from typhoid fever about two months ago. I found her very much emaciated, and confined to bed, from which, I was told, she had not been moved for two months, not even to have the bed made. She seemed to suffer entirely from extreme debility. The temperature was irregular, but generally high in the evening, with slight nocturnal perspiration. The respiratory murmur was normal. The amount of urea excreted during twenty-four hours was 56.05 *grammes* (nearly two ounces). I ordered an infusion of cuca-leaves to be taken when thirsty, and did not see her again for a week, when I found her much improved and able to sit up. She informed me that she experienced "great support" from the infusion for about an hour after taking it; and that, when she found she required further support, she repeated the dose. I had not another opportunity of making an analysis of urea.

I have also found cuca of great service in both acute and chronic pneumonic phthisis. Where there is much febrile

excitement, it lowers the temperature and restrains or materially alleviates the distressing perspiration. In febricula, also, it may be used with great advantage.

I do not, however, wish it to be understood that I regard cuca as sufficient to meet every complication that may arise in specific or other febrile conditions, but would submit that its real value lies in the influence it exerts over that section of the nervous system which controls the *constructive* and *destructive* forces which are constantly in operation in the living body; and that when, as in fever, assimilative nutrition or *constructive* force is in abeyance, cuca restrains tissue-metamorphosis or *destructive* force.

P.S.—Perhaps, for the benefit of those who may wish to try the tincture of erythroxylon cuca, as I found great difficulty in obtaining it, I may mention that I have lately obtained it in large quantities from Messrs. Hewlett and Son, Cree Church Lane, Leadenhall Street.—*British Medical Journal, March* 10, 1877, *p.* 291.

2.—ON FEVER WITH HYPERPYREXIA.

By Dr. C. Handfield Jones, F.R.S., Physician to St. Mary's Hospital.

Hyperpyrexia occurring in cases of fever is, I believe, to be regarded as a functional nerve disorder, analogous to coma, delirium, or convulsions, and dependent on paralysis of the temperature-regulating centres. The existence of such centres seems to me undeniable, in the face of such evidence as can be adduced. It may, however, be questioned whether pyrexia is necessarily dependent on paralysis, more or less complete, of these centres, or whether it may not be brought about independently by chemical changes induced in the tissues and protoplasm of the body by some deleterious agent. Dr. Burdon-Sanderson, in an excellent paper on the "Process of Fever" (vide Practitioner, April, May, June, 1876), states that "two possibilities are open to us. One is that fever originates in disorder of the nervous centres: that, by means of the influence of the nervous system on the systemic functions, the liberation of heat at the surface of the body is controlled or restrained, so that 'by retention' the temperature rises; and, finally, that the increased temperature so produced acts on the living substance of the body so as to disorder its nutrition. The other alternative is that fever originates in the living tissues—that it is from first to last a disorder of protoplasm, and that all the systematic disturbances are secondary." To these two possibilities I add a third, in the form of a modification of the first, which will be stated subsequently. It is

almost identical with that long ago propounded by Virchow. Dr. Sanderson thinks the facts and considerations adduced in his paper "sufficient to justify the definitive rejection of the first hypothesis in all its forms: for, on the one hand, we have seen that no disorder of the systemic functions, or of the nervous centres which preside over them, is capable of inducing a state which can be identified with febrile pyrexia; and, on the other, that it is possible for such a state to originate and persist in the organism after the influence of the central nervous system has been withdrawn from the tissues by the severance of the spinal cord." This latter averment will be noticed subsequently. As to the first, I submit that it is directly opposed to the testimony of Mr. Hutchinson, who uses the term "paralytic pyrexia" to denote the "state of feverishness which is so marked a feature in the stage of reaction after severe injuries to the head or spine. Throbbing, relaxed arteries, and great heat of skin are its chief features; but, in greater or less degree, the other constitutional symptoms usually included in the term pyrexia are present—a furred tongue, dryish mouth, distaste for food, with thirst and scanty urine." In a gentleman, well known to me, whose nervous system is in a feeble state (perhaps in consequence of a strain of his spine), a very little over-fatigue has been for several years certain to be followed by well-marked pyrexia, lasting two or three days. The attacks were attended with shivering and fever, and severe dyspepsia, especially for farinaceous substances. A similar case has been mentioned to me, where the derangement consequent on fatigue was regarded as typhoid fever.

As to the view that fever is from first to last a disorder of protoplasm, and that all the systematic disturbances are secondary, it may be remarked that occasionally cases of typhoid occur where there is no rise of temperature. One such occurred at St. Mary's, under the care of Dr. Sibson. The temperature never exceeded 98°, nor the pulse 90. There were rose spots. Dr. Broadbent had a similar case at the Fever Hospital. Dr. H. Thompson records a severe case, where the temperature did not exceed 101·2°, and the typhoid symptoms were prolonged into a period of normal temperature for nearly seven days. Dr. Johnson (Washington) relates some instances of these low readings when there was no reason to doubt that the patient's disease was typhoid fever. Wunderlich says, page 201 "Medical Thermometry":—"But even when very painstaking observations are made, cases of abdominal typhus do occur, in which, so to speak, the disease remains latent, and where the nature of the fatal illness is first recognised in the dead body. With regard to all these instances it seems

certain that the protoplasm of the body is infected, yet fever does not ensue. Its absence seems to me most reasonably accounted for on the assumption that the temperature-regulating centres, being endowed with unusual resisting power, do not succumb to the morbific agent. The same explanation seems to me the most probable that can be given of those disorders, either non-febrile or but slightly so—as diarrhœa, sore throat, nephritis—which affect occasionally persons who have been exposed to the contagion of scarlet fever, yet who have remained exempt from the specific malady. Again, it is well known how much mental depression or *ennui* promotes the liability to sicken from fevers, and how much the resisting power to morbific influences is increased by an opposite state. The system may be thoroughly impregnated with malaria, yet no fever ensue unless some mental perturbation arise. Sir Samuel Baker says of African remittent fever that any severe action of the mind, such as grief or anger, is almost certain to be succeeded by fever in that country. The part played by the nervous system in many conditions of this kind is sufficiently apparent. It may, so to speak, hold down the temperature if strong and stable, or if weakly and failing allow combustion to run riot. In the latter case it may be greatly aided by the administration of quinine, which is both a nervine and an antipyretic, and the latter because the former. Binz, however, disputes this. Further evidence to the same effect is afforded by intermittent and relapsing fever, and by pyæmia. Here we have paroxysms of pyrexia, with intervals of varying length more or less completely apyretic. It is impossible to suppose that the essential cause of the malady is absent or inoperative on protoplasm during the fever-free intervals; but it is in accordance with what we know of the behaviour of the nervous system to believe that it resists disturbing agencies up to a certain time, and then gives way suddenly. Thus a permanently existing irritation gives rise to sudden explosions of epilepsy, or attacks of asthma, or (as in cases narrated by Trousseau, vol. ii., page 317) to periodic neuralgia.

Murri considers that his experiments have disproved Virchow's doctrine that fever is the result of paralysis of an inhibitory centre situated somewhere in the intercranial nervous system, and Dr. Sanderson seems disposed to agree with him. Murri states that after section of the cord in dogs the skin is less warm, and gives off less heat than normally. This was the case in large as well as in small dogs, and in an atmosphere of 86° Fahr. as well as in a cool one. Even when the temperature of the rectum rose above the normal, in spite of repeated measurements the skin was never observed to become warmer. As the result of certain experiments

in which the heat given off from the bodies of dogs with divided cords (cervical) was measured, he states that in spite of the diminished giving-off of heat the interior of the animals became colder. He accounts for the diminution of production of heat by the paralysis of the muscles, by the slackening of the circulation (the blood stagnating and accumulating in the abdominal and other vessels), by the diminished work of the heart, and by the diminution of the internal friction of the blood-current. When fever was produced in his dogs *before* the division of the spinal cord, the operation had not the above-mentioned cooling influence. Also when the cord was first divided (which produced cooling), and putrid pus then injected, the temperature rose just as in animals not operated on. Murri maintains on all these grounds the bio-chemical theory of fever, according to which pyrogenic substances produce abnormal chemical decompositions.

I have examined the details of Murri's experiments as given in his essay, "Sulle Teoria delle Febbre," and am inclined to go further than Dr. Sanderson does, though he considers them hardly conclusive. His first experiment certainly shows that section of the cord between the sixth and seventh cervical vertebræ may produce very decided fall of the temperature, both in the rectum and under the skin of the thigh. The temperature of the rectum fell in two hours 3·4° C., of the skin 2·3° C.; in six hours the rectum fell 5·5° C., the skin 3·9° C.; in twelve hours the rectum had lost 6·7° C., and the skin 5·5° C. The temperature of the room throughout was 23·5° C. But in Experiment 14 the rectal temperature rose in five hours 0·4° C., while the cutaneous remained the same; in fourteen hours the rectal temperature had fallen 0·4° C., and the cutaneous 0·3° C.; in twenty-six hours the rectal temperature had fallen 1·2° C., and the cutaneous 1·2°; in thirty-six hours the rectal temperature had risen again, and was only 0·3° C. below the reading before the section of the cord, the cutaneous being 0·6° C. The temperature of the room throughout was 21° C. In Experiment 13, after the lapse of more than twelve hours, the rectal temperature had risen 2° C., and the cutaneous 1·8° C. Here, however, the temperature of the room was higher, varying from 25° to 30° C., and this may have hindered the cooling. But in Experiment 14 this could not have been the case, as the room was colder than in Experiment 1, and yet the fall of temperature was comparatively trifling. It certainly does not appear proved that section of the cervical cord necessarily causes a notable lowering of the temperature. But a much more potent objection to Murri's conclusion is that in the human subject the temperature of both skin and rectum are often raised very much indeed by destruction of the cervical

cord. Moreover, it may be that in Murri's experiments the section—which was always between the sixth and seventh cervical vertebræ—was made too low down, for both Tschechichin and Schreiber agree that a narrow area near to the junction of the medulla oblongata and the pons is the part where injury is specially effective in producing high temperature.

Murri's experiments relative to the amount of heat given off by animals whose cervical cord has been divided seem to me more conclusive. He shows that a sound dog placed in a box not only kept up his own temperature, but warmed the air of the compartment 6·7° C. in one hour and fifty minutes; while the same animal when his cord had been divided lost in one hour and forty-five minutes 2·1° C. of his own temperature, and only warmed the air of his box 5° C. This certainly seems to prove that a sound organism gives off more heat than a paralysed one. Yet, on the other hand, it seems hard to believe that a human body whose surface marks a temperature of 110° does not give off more heat than one at 98°.

In support of the statement that, when fever was produced in dogs by the injection of pus, the subsequent division of the spinal cord did not cause cooling, Murri gives two experiments. In Experiment 15 the temperature of the rectum rose 0·6° C. after pus was injected, and 0·5° C. more after the cord was cut, then gradually declined in six hours to 39·8° C. This was 0·8° C. above the starting-point before pus was injected, but is not a febrile temperature for dogs necessarily (*vide* experiments 14 and 11). The temperature of the skin of thigh rose after the cord was cut 0·5° C., then fell. The total loss in six hours was 3·6° C. A further fall in the temperature of both rectum and skin might have occurred had the experiment been prolonged. The temperature of the room was 20·5° C.

In Experiment 16 the rectal temperature was raised 0·8° C. by injection of pus, then 3° C. by struggling. After section of the cord it fell in four hours to 38·6° C., while the posterior paralysed limb fell to 31·7° C., and the anterior to 29·8° C. The loss in the rectum was 0·8° C., in the posterior limb 0·5° C., and in the anterior 4·6° C. The temperature of the room was 19° C.

These results do not seem to me to warrant the above statement.

Again, Murri avers that putrid pus produces fever when injected subcutaneously after the cord has been divided, and cooling thereby occasioned. Experiments 18 and 19 are given in proof. In the former the temperature of rectum fell from 38·5° to 36° C., and the cutaneous in thigh from 36° to 33·5° C. in three hours (pus was injected one hour and a half before the

end of this time.) The temperature then rose and kept at 38° C. or a little above for thirteen hours in the rectum, and at 34° C. or a little over in the skin. The temperature then fell, and in eight hours descended to 33·5° C. in the rectum, and 30·2° C. in the skin of thigh. The temperature of the room was 19° to 20° C. In this instance, though some elevation of temperature followed the injection of pus, yet it never reached fever height in rectum or skin, and soon fell again considerably. The highest point reached was 38·9° C., and this only once.

In Experiment 19 the cord was cut at 10.15 a.m.; at 10.20 temperature of rectum 40·3° C.; of thigh (subcutaneous) 38·2° C. At 11 a.m. fetid pus was injected. Temperature of rectum fell to 38·9° C.; after 2.15 p.m. began to rise, but did not reach 40° C. till 9.50 p.m.; it remained at 40·1° to 40·4° C. till 5 a.m. of second day, when it fell 4° or 5° C. for five hours, then rose again, and reached 40·7° C. (its highest point) between 11 and 12 a.m.; again fell some tenths until 5.30 p.m., again rose and remained at 40° or 40·1° C. till 8.45, when the experiment closed. The temperature of skin of thigh at 1 p.m., 36·1° C.; at 9.50 p.m., 37·3° C.; at 10.30 a.m. of second day, 37·4° C.; at 11.25, 38° C.; and continued at same figure, or a little above, till 2.20 p.m.; then declined a little, rose again at 5.15 p.m. to 38° C.; at 7.45 p.m. reached 38·8° C. (its highest point), and fell to 38·1° C. at 8.45 p.m., when the experiment was concluded. Temperature of room 24° to 25° C. In this instance the further fall of the temperature, which had commenced after section of the cord, was checked by the injection of pus, but the temperature produced by this was only 0·4° C. in the rectum, and 0·6° C. in the skin above that noted five minutes after division of the cord. These are the maxima, but, reckoning from three hours after the injection of pus, the average height of eighty-four observations made in thirteen hours amounts to 39·5° C., and that of ninety-seven made in the succeeding seventeen hours to 40° C. for the rectum. The corresponding amounts for the skin are 36·7° and 37·9° C. If these are compared with those of the starting-point — viz., 40·3° C. for the rectum, and 38·2° C. for the skin of thigh, it is seen that there is a decline during the first period of 0·8° C. in the rectum, and 1·5° C. in the skin. In the second period the decline for the rectum is 0·3° C., and for the skin 0·3° C. It is clear, therefore, that the pyrogenic effect of the pus did not avail to bring up the temperature to the normal, and consequently the result was not such as in animals with sound cords. Murri says that the dog in this experiment continued to live for four days more, the temperature still remaining high. This leads one to question whether the injected pus was the cause of

the rise in temperature, for the pyrexia produced by pus in dogs usually lasts about two days. Perhaps the section of the cord was the real cause. This last point, then, like the preceding, seems to me by no means made out.—*Med. Times and Gazette, March* 10, 1877, *p.* 254.

3.—NOTES OF AN EPIDEMIC OF TYPHOID FEVER, WITH SPECIAL REFERENCE TO ITS ANTIPYRETIC TREATMENT.

By Dr. George Hunter, Linlithgow.

Linlithgow Bridge is a village of 465 inhabitants, situated on both sides of the river Avon (which here divides the counties of Stirling and Linlithgow), rather more than a mile to the west of Linlithgow. The houses are, with few exceptions, old, and their sanitary arrangements in every respect defective. The drainage is entirely surface, by means of gutters which run along each side of the village-street towards the river. The excreta, human and bestial, are carried to midden-steads in the gardens, at the back of and in close proximity to the houses. In many instances, this ground is on a higher level than the houses themselves. Free porous gravel, overlying trap formation, forms the subsoil.

The *water* supply is from wells public and private, scattered over the village. That from which the great majority of the inhabitants on the Linlithgowshire side of the river derived their supply, was known as the "Brae Well," and had the reputation of being the best in the place. The wells known as M'Arthur's, Miller's, and Lockhart's, were made use of by those residing in their immediate proximity, especially when it became known that the Brae Well was pronounced impure. On the Stirlingshire side, the "Wester Well" was the one almost exclusively had recourse to.

The *milk* supply was obtained from six different dairies. In three of these, viz., those of Braes, Battison, and Bennie, typhoid fever early made its appearance.

For at least ten years previous to the present epidemic, there had been no serious outbreak of any zymotic disease in this village. Now and again, isolated cases came under my notice, the last of which I attended in February last, and it proved to be a mild case.

Mode of Origin and Propagation.—On the 17th July last, I was called to see Mrs. L., a young married lady, and found that she had been ill for a week with unmistakable symptoms of typhoid. This was undoubtedly the first case of what subsequently proved a very serious epidemic. The villa in which Mrs. L. lived was at a considerable distance from any of the

other houses, and on this side of the river. It was comfortable and commodious, and the sanitary arrangements in every respect satisfactory. To confirm my own examination, a skilled workman was brought from town, who inspected the trappings of the drains and water-closet, and the joinings of the glazed pipes (the only ones in the village) which conveyed the sewage to the river, and no flow could anywhere be detected. There was no accumulation outside in the shape of a middenstead, for such a depôt did not exist in Mr. L.'s residence, the ashes, &c., being conveyed daily to the dungstead of an adjoining small dairy farm, occupied by Mr. Braes. The milk supply was from this dairy, and all the members of the Braes family were at this time in good health. The water supply was from a private pump-well, and was found, on subsequent examination, to be free from anything like injurious contamination.

There was no other person ill of fever in the village, and the case which had occurred early in the spring was on the other side of the river, no communication having taken place in the interval between the two families.

Mrs. L. was delivered of her first baby in the beginning of June last, and made a good recovery. With the exception of having been twice to Edinburgh for an hour or two's shopping, and *subsequently*, once at church, she had not been anywhere from home since her confinement. In her case, the fever ran a severe and complicated course, and she succumbed to it on the 5th August.

The next case appeared in the Braes family, whose house was on the other side of the road, about 40 yards further north than Mr. L.'s from Linlithgow Bridge. There was frequent inter-communication between the two houses; on the one hand, by taking milk to the villa, on the other, by conveying ashes and certain excreta to the dungstead. Previous to being attacked, this patient had been employed about the dunghill, shovelling up and covering over what had been scratched about by the poultry.

Regarding the sanitary arrangements of this dairy farm, a few words are here necessary. The byre was at the end of and attached to the house, communication with the kitchen being obtained by a door almost directly through the gable; and through this door the odour of cows and cow manure was constantly perceptible. The dungstead was placed at the end of the line of houses, and the ground was slightly excavated for its reception. It was but seldom entirely cleaned out, and a stream of liquid sewage constantly flowed into the loose gravelly soil of the adjoining field. About 40 yards in a straight line west from this dungstead, and situated at a much lower level, was the Brae Well already referred to.

The next case was that of a young man named Bell, who, happening to be passing along the road on the day the lad Braes was working at the dungstead, stopped and had some conversation with him, and he became affected with typhoid on the same day that Braes was seized.

There were now three distinct *foci* of infection :—

1st, *The villa.*—Fever broke out in the house of the person who nursed Mrs. L.'s infant, and spread to the neighbouring houses; and also in the coachman's family, in which no fewer than six persons were affected. (While still ill of fever, his wife was delivered of a well-nourished female child, and both made good recoveries.)

2d. *The young man Bell's house*, from which several families living in the immediate vicinity were early affected, and of these one died.

It may be here interesting to note, that to this focus the first appearance of the fever on the Stirlingshire side of the river was distinctly traceable. In the house next to Bell's lived a family named Allan, and during the convalescence of Mrs. Bell (who became ill after her son) Mrs. Allen was seized. In the course of her illness she was visited by her mother-in-law, who resided at a considerable distance across the river. Soon after having been in her daughter-in-law's house, Mrs. Allen, sen., exhibited undoubted symptoms of typhoid, and passed through a severe attack of the disease.

Following closely upon old Mrs. Allan's attack, fever broke out in the houses of Bennie and Battison—the only persons who kept dairies in this part of the village; and from these, and the "Wester Well" water, fever soon became general on this side of the river.

An analysis of this water was now made by an eminent chemist, and the result has here been placed before you.

Saline matters, ...	15·92	grains.
Organic and volatile matter ..	2·24	,,
Hardness	70	,,
Nitrates decided in million parts.		
Saline ammonia	0·072	,,
Albumenoid ammonia	0·067	,,

3rd, *The house of Braes, the dairy farmer.*—In this family no fewer than six persons were struck down with the disease, and, with one exception, they were all tedious and dangerous cases, Mrs. Braes succumbing to a pulmonary complication. In all these cases diarrhœa was a troublesome and marked symptom, and the evacuations, latterly after disinfection, were thrown out upon the large dungstead, which, after a fall of rain, became a pond, and soaked the soil of the field to a considerable

extent. It is, therefore, more than probable that the "Brae Well" got polluted from this source (as pointed out by Dr. Littlejohn in his excellent report to the Local Authority), for Dr. Stevenson Macadam's chemical analysis of water taken from it proved that it was contaminated with the products of the decomposition of organic matter of the nature of sewage, and that it should not be employed for cooking or drinking purposes.

The analysis in full I here place before you.

Saline matter	16·81	grains	In imperial gallon.
Organic and volatile matters ..	2·43	,,	
Hardness..	80	,,	
Nitrates, decided.			
Saline ammonia	0·074	,,	In million parts.
Albumenoid ammonia	0·070	,,	

Milk from this dairy was supplied more extensively than from any in the village, and a great many families who subsequently became ill of fever traced it to this source. Churning and buttermaking were carried on within a few yards of those lying ill of fever, and the same person who nursed them managed the dairy.

I may here mention, in passing, that I was able to fix with certainty the incubation period in one of these cases. A member of the Bennie family came home to Linlithgow Bridge from his situation as farm-servant in a distant part of the country, and spent Sunday with his friends. He partook of dinner and tea prepared with water from the infected well, and returned to the farm in the evening. For two weeks after, he remained in his usual health. On the beginning of the third week he had the prodromata of fever, and by the end of the same he returned to Linlithgow Bridge, had a rigor, and passed steadily through an attack of the complaint. There was no suspicion of fever anywhere in the neighbourhood of the farm at which he was a servant. The duration of the period of incubation was thus twenty-one days, which includes the time of the prodromata.

It was now considered highly desirable to have the water from the other wells submitted to reliable analysis. This was carried out, and in every instance, if we except that of Mrs. L., the water was found to be contaminated with sewage or organic matter, and pronounced unfit for domestic consumption.

With such favourable conditions as filth accumulations, and impure water and milk supply, was it to be wondered at, that the fever poison spread with alarming rapidity and severity, and that out of a population of 465 inhabitants, including both sides of the river, 68 should become affected—being upwards

of 14 per cent. of the whole population (I speak of my own practice only)? Of these 7 died, being a mortality of rather more than 10 per cent. of those affected.

Briefly taking a retrospect of the prognosis of the epidemic, one is confronted *in limine* with the question, how did the first case, that of Mrs. L., originate? Did it begin *de novo* in her own house, or was it the result of contagion from some pre-existing case?

In the minds of the profession much difference exists regarding the mode of origin and propagation of the disease. By some physicians it is believed to be purely contagious. "It is now about 30 years," writes Dr. Aitken, "since M. Bretonneau related to the French Academy of Medicine a series of cases in which the communication of this disease from person to person, and its modes of propagation in this way, were so evident as to admit of no reasonable doubt." Further on he quotes Dr. Watson: "If this fever be really contagious, it is not only erroneous but dangerous to hold the contrary opinion." Murchison, on the other hand, is strongly of opinion that it originates in the decomposition of organic substances; whilst Niemeyer considers that it extends by contagion as well as miasm, or that the germs which cause abdominal typhus may develop and multiply as well in the organism of a typhus patient as they would under favourable circumstances outside of it. But he thinks it doubtful if the contagion be transferred by the exhalations from the skin and lungs. Finally, Liebermeister, who gives no uncertain sound, and is much more exact in his opinions, states, "We must recognise on the one hand that typhoid fever is never contagious from person to person; and on the other hand, that it never originates spontaneously, but by a continuous transmission of the poison. The disease is not contagious in the proper sense of the word, for it is never transmitted by direct contact. It is not purely miasmatic, for external conditions alone are not sufficient to produce it. The presence of a person suffering from this disease, or of substances derived from such a person, is necessary. The poison is propagated continuously. It travels from the diseased individual to the localities which are favourable for its growth and multiplication, and from these localities again into the human body."

If there be this diversity in the opinions of medical authorities, to whom can the laity look for counsel and direction to guide them in the emergency of an epidemic? Eager attempts at disinfection are made by non-professional persons, but where, how, and what to disinfect they know not; and as a result we find that chemicals with pungent and penetrating odours have been scattered broadcast where little if at all

required; and where disinfection is really required, none has been had recourse to.

That typhoid fever can be transmitted directly from person to person by exhalations from the lungs or skin—*i.e.*, that it is truly contagious in the sense that smallpox and scarlet-fever are—is believed by a great section of the community, and it is to be feared by not a few members of our profession. But such a view is rendered untenable by the fact, that in several instances I found only one, or perhaps two, affected out of families comprising six or seven individuals, and where no attempt was made to prevent the most free communication between the affected and healthy. Care was always taken, however, to disinfect promptly and completely the dejections immediately after their evacuation by the patient. Similar evidence is furnished in Fever Hospitals, where the physicians, attendants, and nurses taking charge of such cases are not more frequently affected than those who never come in contact with fever patients.

Nor does the pythogenic hypothesis of Murchison afford anything like a satisfactory explanation of all the facts and circumstances observed in the course of this epidemic. For here we have a village in the worst possible hygienic condition, with accumulations of animal decomposing substances, notably made up of excrementitious matters from byres and piggeries, as well as fæcal masses under their windows and outside their doors, a loose gravelly soil saturated with liquid sewage from such accumulations, and yet for ten years at least the public health of the inhabitants has proved itself invulnerable against any attempt at an outbreak of fever worthy the name of an epidemic. On the other hand, in the house of Mrs. L. we have the most perfect sanitary conditions, free ventilation, and efficient drainage, water pronounced to be innocuous after analysis by an eminent chemist; all the conditions, indeed, which sanitary science and medical officers of health desire to obtain in order to prevent the appearance of fever, and to arrest its spread when it has commenced, and yet it is from this house that the first case takes its origin.

From a consideration, therefore, of all the circumstances and conditions which obtained in this epidemic, we are forced to the conclusion, that the fever did not originate *de novo* in Linlithgow Bridge, but was contracted by Mrs. L., either on her way to or in Edinburgh (from which cases of fever are seldom absent), and that from her it became propagated continuously by the alvine evacuations. From this source every facility was afforded for the admission of the infective germs (the *ectogens* in contradistinction to the *entogens* of smallpox and the like) to the water, milk, and butter supply, by percolation through the

porous soil from dungsteads, by pitchers with adherent ectogens being dipped into open wells, and by direct impregnation in the dairies. If it be true, as Mr. Simon affirms, that "in any specific diarrhœa, such as eminently is enteric fever, every discharge from the bowels must teem with the contagion of the disease," the practical lesson for us to inculcate upon the minds of the community is, that every discharge from the bowels of a typhoid patient should be submitted to thorough and reliable disinfection immediately on its expulsion, and that it should afterwards be buried and covered with chloride of lime in a place remote from the possibility of contaminating drinking water or its source.

It must be evident that no treatment can be regarded as rational or scientific which has not for its object the reduction of the abnormally increased temperature, and, as far as possible, the prevention of its production. The cold bath, and such remedies as quinine, salicylic acid, digitalis, &c., are the means to which we trust for accomplishing this end; and to the German physicians, Jürgensen and Bartels, Liebermeister, Niemeyer, and Riess, we are indebted for what we know of the antipyretic action of these agents in the treatment of typhoid.

These observers have found, that after frequently-repeated baths, commencing with a temperature of 68° F. (thermometric observations being regularly recorded), there is a decided decrease in temperature, and a marked remission of the more serious symptoms.

In an epidemic such as I have described, this mode of treatment can never become general (except, perhaps, in the case of children), both because of the want of suitable accommodation and appliances, and from the difficulty of obtaining qualified intelligent nurses to carry out the treatment prescribed. When we find that as many as twelve baths are sometimes required in one day, or more than two hundred in the course of treatment of one single case, I need not say that, in country practice at any rate, the time has not yet come for its general adoption.

It is now more than twelve months since I became aware of the antipyretic effects obtained by Liebermeister and Vogt after the employment of large doses of quinine. Until recently, I had few opportunities of putting these observations to the test, but in several cases in which I had recourse to its use during the past summer, the effects were to me sufficiently striking. In one patient—the first so treated—the disease had reached the middle of the second week when I found an evening temperature of 105·5° F., accompanied with delirium, catarrh of the smaller bronchi, and frequent cardiac action.

With no little hesitation, I prescribed 45 grains of the sulphate of quinine, in divided doses of 7½ grains every ten minutes, and anxiously awaited the result. The following morning the temperature had fallen to 100·3° F.; the brain acted normally if slowly; and the pulmonary affection had almost entirely disappeared. Although the temperature subsequently rose, it never reached the same height, and the patient made a good recovery. Convinced, then, of its utility, I have employed it in a considerable number of severe cases, with the result (with very few exceptions) of reducing a temperature of 104°-105·5° F. to 100° F., and even to 98·5°; and with this decrease, a marked improvement in the general symptoms. To obtain such a result, large doses must be given at brief intervals—or it may be given in one single dose—for quinine is rapidly eliminated by the kidneys. For adults, I order, as recommended by Liebermeister, 45 grains divided into 6 powders, one of which is to be taken every five or ten minutes. Where I have considered the effects of this dose insufficient, I have ordered as much as 50 grains, without observing any bad effects beyond the physiological action of the medicine, and perhaps vomiting with the first or second dose. In addition to the ringing in the ears and increased deafness, I have observed in several cases, especially in young people, marked dilatation of the pupil, and in nearly all, general paleness of the cutaneous surface.

For every case which threatened to be at all serious in the course of this epidemic I had a chart drawn out, on which the temperature and frequency of pulse, number of stools, &c., were carefully noted at the bedside, morning and evening.

In most cases, when the temperature reached 103·5° F. in the morning, or 104·5° F. in the evening, quinine in antipyretic doses was administered. Latterly, however, finding that a temperature slightly lower, but more continuous, produced equally serious threatenings of cardiac paralysis, as evidenced by increased frequency and weakness of pulse, and hypostases of the lungs, I had recourse to the remedy even before this height was reached. Whether it be administered in the morning, as suggested by Binz, or in the evening, as recommended by Liebermeister, appears to be immaterial, for, at my next visit, a marked reduction of temperature was always sufficiently evident. More than one dose was always required; but it was seldom necessary to repeat it oftener than once in two days, and frequently not so often.

We have thus in quinine a potent and trustworthy remedy, possessing nearly all the efficacy of the cold bath, and yet almost universal in its application. In some cases, however, there is a peculiar intolerance of it, even in much smaller doses than those mentioned; and this, together with its high price,

makes it desirable to have at our disposal a medicine which is equally safe and trustworthy as an antipyretic, and yet procurable at a more moderate cost. In salicylic acid, I believe, we have such an agency.

Without having been aware of the published observations of Riess of Berlin on the action of salicylate of soda in the treatment of this disease, I was led to make trial of the acid from my experience of its beneficial action in rheumatic fever.

It may be employed in the form of (1) *salicylic acid*, which is an insoluble yellowish white powder, without odour, and of a sweetish taste. It is readily soluble in water containing an alkali in solution, and thus, with soda, we obtain (2) *salicylate of soda*, a neutral salt. (3) As *salicylate of quinia* I have also used it, but in this form it is expensive, and not more antipyretic than either of the preceding.

It may also be prescribed in the form of ammonia or potass salts; but the lowering action of the potass on a heart already feeble seemed to contraindicate its use in the only case in which I had recourse to it. My experience leads me to prefer the soluble soda salt, since we now know that, in the presence of the carbonic acid of the blood, salicylic acid is set free.

Like quinine, it must be given in full and frequently-repeated doses. For an adult, 10 to 20 grains are given every ten minutes until 6 doses are taken. For a child of 5 years I have given as much as a drachm and a half in an hour without any toxic effects resulting; but in two instances—both children—slight croupiness of inspiration followed, probably the result of catarrh of the pharynx and upper part of the larynx. Abundant diaphoresis has attended its employment in two or three of my patients. Tried at intervals of one or two hours, no antipyretic action was noteworthy, proving that it is rapidly eliminated. In nearly all the cases treated with salicylic acid, the pulse fell with the temperature, delirium of a mild character gave way. Symptoms of pulmonary œdema and hypostatic congestion became less anxious complications, and in these respects I found a decided resemblance to the action of quinine. Salicylic acid is, moreover, a valuable astringent, and is therefore peculiarly indicated in those cases where the diarrhœa is frequent and exhausting. After its use the dejections become more consistent, and usually do not exceed one or two in the 24 hours. This of itself is a strong point in its favour, since the chances of the formation of bedsores are lessened, the duties of the attendants are lightened, and the risks of propagating the disease are *a priori* diminished.—*Edinburgh Medical Journal, Feb.* 1877, *p.* 693.

4.—SALICINE AS AN ANTIPYRETIC.

By Dr. John A. Erskine Stuart.

Salicine is a substance which for a long period of years has been regarded as an antiperiodic and tonic, and it has consequently been used considerably in the treatment of intermittents and diseases of a neuralgic nature, where there is a tendency to plethora or congestion and head symptoms, as it has the advantage over quinine in not producing ringing in the ears and the other symptoms due to a full dose of that drug.

Of late, however, this drug has been extensively used in the treatment of rheumatic fever, and we have it on the testimony of several observers, but especially of Dr. Maclagan of Dundee, that it acts quite as efficaciously in this disease as its near relation salicylic acid, and besides has the advantage of being easily administered. Dr. Maclagan is of opinion that it is split into some substance in the system which possesses powerful antiseptic properties, and we have it asserted by Senator, Ranke and Lehmann, and Millon, that it is converted into salicylic acid in the blood. Whatever substance is formed in the blood, I have proved to my own satisfaction, by two experiments, that salicine in solution has almost no power in arresting the putrefaction of urine, while if it is taken internally in even a small dose, it gives the urine a power of resisting putrefaction for a considerable period. I find that a solution of five grains of salicine in half an ounce of water introduced into one ounce and a half of fresh urine only preserves that fluid for half a day longer than the time at which urine left to itself in similar circumstances went putrid. However, urine passed one hour after taking a dose of ten grains of salicine kept fresh and clear for five days. Salicine is excreted in the urine as salicyl hydride, easily detected by the purple red colour produced on adding perchloride of iron.

In conclusion, I may state that neither quinine nor bebeerine possess antiseptic properties worth mentioning when added to urine, but it may be that their powerful antipyretic effects are due to their transformation into some antiseptic substance in the blood.—*Edinburgh Medical Journal, Dec.* 1876, *p.* 539.

5.—ON SOME OF THE SEQUELS OF TYPHOID FEVER.

By Sir James Paget, Bart., F.R.S.

I venture to call attention to the frequent occurrence after typhoid fever of these following diseases, namely, phlebitis, periostitis with or without necrosis, especially periostitis of ribs, local paralysis of muscles.

Of the frequency of these diseases after typhoid fever I can

have no doubt. I believe that I have seen, taking all together, more than seventy cases in the last ten years. I do not feel competent to deal with the question whether each fever has, as seems very probable, its own proper sequels, and is in this sense, though perhaps in less degree, as specific as in its fever-period; but, excepting the phlebitis, I have not yet seen any of the diseases I have enumerated after any other than typhoid fever, or a fever which I suppose to be closely related to it; and I have not seen after typhoid any corresponding number of cases of large lymph-glands, diseased joints, or other diseases of mere debility such as may follow any acute illness.

The fever which I have referred to as "closely related" to typhoid, is that which in the last two or three years has been prevalent in Naples, Rome, and I believe other places in middle and southern Italy. Some call it typhoid; Neapolitans often call it Roman, Romans Neapolitan; in some cases I have ample reason to believe that it was genuine typhoid; and the likeness of its frequent sequels makes me ready to believe that it always was so.

It is common to all these sequels of typhoid fever that they appear when the patient is considered to be "well of his fever," when his temperature is normal, and I believe constant, when he is beginning to move about and becoming stouter and stronger. I do not remember to have seen or heard of a case in which any of them occurred during the continuity of the fever.

The *phlebitis* has in it no character widely different from those in which it is seen in other of its so-called adhesive forms. Its most usual seat, by very far, is in the femoral, especially the left femoral, vein. It is generally marked by aching and tenderness along the line of the inflamed part of the vein, with stiffness at the groin, and more or less of firm general swelling of the limb. There is no marked access of fever, no other disturbance of the general health than may be ascribed to the local distress. The duration of the painful symptoms is, I think, quite uncertain; but after they have ceased, some bigness and clumsiness of the limb usually remain for many months. In many cases the limb remains always larger than the other, but not weaker or less muscular; and in some, in which, I suppose, the femoral vein is in more than the usual length obstructed, the superficial veins become very large and tortuous.

I have not seen or heard of a case in which suppuration attended the phlebitis, or in which embolism occurred; and I do not know of any in which the affected veins were examined after death. I believe that the condition is one of inflammatory swelling and thickening of the walls of the vein, and consequent narrowing of its canal. Very probably, also, there is lymph-

exudation or deposit in layers from the blood. I doubt whether thrombosis is ever a primary event, though it may occur as a consequence of the inflammatory changes in the lining of the vein.

The *periostitis* following typhoid fever has its most frequent seat on the tibiæ, but I have seen it on the femur, the ulna, the parietal bone. In one case it was symmetrical on the lower parts of the shafts of the tibiæ; but, with this exception, I have not seen it on more than one bone in the same person. It has always been circumscribed within a space of from one to three inches in extent, has always occurred in the convalescence, has never been attended with the delirium, fever, or other severe symptoms attendant on acute necrosis. At first it is only marked by the limited, hot, and very tender swelling of the periosteum, and I have seen cases in which the periostitis has remained very long in this condition, and then slowly subsided without necrosis or any other abiding change of structure. But in two cases, at least, the patient has remained for months —in one case for three years—subject to repeated attacks of pain and swelling of the periosteum, yet without signs of suppuration or more change than a slight permanent thickening.

When the periostitis has been associated with necrosis, the extent of the dead bone has always been far less than that of the inflammation over it, and, with rare exceptions, only the hard compact structure of wall or outer table has perished. The separation of the dead bone has taken place in the usual way, and has been followed by healing, which, so far as I have seen, has been permanent.

The *periostitis of the ribs* is in so much unlike the kind just described, that I have never yet seen it associated with necrosis. It so nearly resembles the ordinary scrofulous periostitis of ribs, that I have sometimes thought it should be regarded too as only an evidence of scrofula educed by the feebleness of nutrition consequent on the fever. Yet I have never seen it associated with other similar signs of scrofula, and it has occurred, after typhoid, in some of so robust and apparently unblemished constitution, that it would seem absurd to impute scrofula to them.

In the usual course of this disease, beginning always, I believe, in the convalescence, a well-defined round or oval swelling is found on one or more ribs. Whether on bone or cartilage, the swelling is generally on the front of the chest. It is painful and tender, and suppuration slowly ensues in it. Then slowly it discharges itself through one or more channels leading obliquely through the overlying structures and through small openings in the skin. The flow of pus, thin and pale, continues usually for many months; I have known it continue

for even two years; and not rarely fresh openings are formed for branches from the first channel of discharge. In the end complete healing takes place, with deep depressed scars and complete clearing-up of all the periosteal swelling.

I have never known necrosis of a rib in these cases, nor ever seen or heard of any advantage from laying wide open any of the sinuses. The general health never appears to be seriously affected by the abscesses, unless in persons of naturally feeble health. Commonly, indeed, the patients feel quite well, and can be active, suffering nothing more than slight local inconvenience.

All the cases of this sequel of typhoid fever that I have seen have been singularly alike, varying only in the position of the abscess. In one, in which I believe that the disease began in the periosteum of the 11th or 12th rib, a great abscess slowly made its way between the abdominal muscles, and I had to open it at the groin. In that case alone have I seen life endangered.

Local paralysis of muscles in the convalescence of typhoid I have seen in only a few cases, and in these the muscles affected were those supplied by the peroneal or the anterior tibial nerve. Once, also, I have seen paraplegia; but I might have doubted whether the paralysis were in more than some accidental connection with the typhoid fever, if it were not that the frequency of nervous diseases in the convalescence of typhoid has been abundantly proved by others, especially by Dr. Northnagel. He has seen or found on record cases not ouly of local motor paralysis, but of hemiplegia, paraplegia, some muscular tremors and spasms, anæsthesia, hyperæsthesia, and neuraglia.—*St. Bartholomew's Hospital Reports*, vol. xii., 1876, *p.* 1.

6.—ON THE MANAGEMENT OF THE BOWELS IN ENTERIC FEVER.

By Dr. Thomas W. Grimshaw, M.A., Physician to Steevens' and to Cork-street (Fever) Hospitals, Dublin.

The question of the management of the bowels in enteric fever has been one upon which different opinions have prevailed among the highest authorities. Thus Dr. Todd says:—"Restrain diarrhœa and hemorrhage in typhoid fever, and when you have fairly locked up the bowels, keep them so. Patients will go for four or six days, or even longer, without suffering inconvenience from this state of constipation." Other writers, Drs. Johnson, Gardiner, and Maclagan, recommend "laxatives."

Dr. Murchison says:—"When there is constipation at the

commencement of the attack, it is well to commence the treatment with a small dose of castor-oil, or rhubarb in peppermint water;" and when the bowels are confined at a later stage, I am in the habit of prescribing, every second or third day, one or two teaspoonfuls of castor-oil, or a simple enema. But when constipation succeeds to severe diarrhœa, the best practice, I think, is to abstain from interfering for four or five days, and then only to prescribe a simple enema, or one teaspoonful of castor-oil."

The practice of using purgatives in fevers generally is considered by Dr. Hudson, who says:—"Among the worst cases admitted into hospital are those of patients who have been dosed with salts by themselves or their friends."

This sentence was written by Dr. Hudson in 1867, or now nearly ten years ago; it was repeated by him in the new edition of his work in 1868, and yet this dangerous practice of administering violent purgatives to patients at the commencement of fever is pursued not only by the patients and their friends, but even by some practitioners of medicine in Dublin and elsewhere, who ought to know better. I believe the injudicious use of astringents at the commencement of enteric fever is almost, if not quite, as injurious as the administration of purgatives. The administration of a violent purgative to a patient suffering from enteric fever in its early stage has this advantage to the patient over the use of an astringent—viz., that it effectually prevents his going about, and soon drives him to bed or to hospital, where he has at least some prospect of rest and quiet, and generally of more judicious treatment. The astringent treatment, on the other hand, has generally the effect of enabling the patient to go about a little longer, and thus more quickly use up his failing strength, gradually drifting him into a state of established disease, which is certain to be of long duration, terminating in tardy or imperfect convalescence, and not infrequently in death. The number of cases of enteric fever which have come under my notice where injury has been done by the injudicious use of astringent or purgative medicine, is almost incredible.

[The following case is given, amongst others mentioned as examples, by Dr. Grimshaw.]

A lad aged about sixteen, admitted to Steevens' Hospital, stated that "he had had a looseness of bowels; went to a doctor who *stopped* it; he never was bad till his bowels stopped." He then came to Steevens' Hospital, where he was found to have a swollen tympanitic abdomen, tender *all over*, dry brown tongue, the characteristic rash, and all the symptoms of severe enteric fever of about fourteen days' duration; his

bowels had now been confined for a week. He was ordered one teaspoonful of castor-oil, with six minims of tincture of opium, and if action of the bowels did not take place in a few hours, a simple water enema was to be given. The enema was not required. The oil acted well, effectually relieving the tenderness and distension; the amount of fæcal matter passed was so large and so offensive, and followed by such considerable depression, that some doubt was entertained as to the ultimate result to the patient. He made a very slow recovery, the iliac tenderness remaining much longer than usual.

It is by no means easy always to diagnose a case of enteric fever in its earliest stage; but no one should ever venture upon active treatment until the nature of the disease has been made out. The frequency with which a "looseness of the bowels" or a "gastric attack" has been treated simply *as such* by astringents in one case, or "a good clearing out" in the other, without any careful attempt having been made either to determine the cause of the looseness, or the nature of the gastric attack, is a disgrace to the profession of medicine. In many instances, this looseness of the bowels, or gastric attack, is the commencement of a serious attack of enteric fever, which, if carefully diagnosed and treated accordingly, might have terminated favourably.

We know that patients walk about at the commencement of and not infrequently during the greater portion of the course of an attack of enteric fever, and some persons have excused an error of diagnosis upon the ground that the patient came to their house to consult them, and had *only* a furred tongue, a quick pulse, and had been in the same state for a week. It may be the result of my peculiar training, but I must say I would assume that a patient with a quick, weak pulse, furred tongue, loss of appetite, and who had been in the same state for a week, had enteric fever, unless there was good cause to believe to the contrary. If I find, on inquiry, that he had been chilly, had vomiting, pains, and either constipation or diarrhœa, and irritation of the urinary organs, I would consider my diagnosis almost certain, even before I had an opportunity of testing the temperature and examining for an eruption, or ascertaining the nature of the discharge from the bowels. Careful observation has convinced me that some practitioners stop short after the discovery of the quick pulse and furred tongue, and only inquire for a headache or for the state of the bowels. If the bowels are confined, the inevitable purgative follows, which is only regulated in intensity by the length of the constipation and the habit of the patient. If the bowels are too free, then as certainly follows the astringent. In a few days more the patient is on the brink of the grave from enteric

fever. In other instances the practitioner explains to the patient or his friends that he has "only got gastric fever." Now this is a course which should be scorned by every educated physician. He should, in every case, state, emphatically and distinctly, that the disease is *typhoid fever*, which is the term the public are familiar with as the title of this very severe and treacherous form of disease. I believe it is scarcely excusable for a physician to mistake enteric fever for merely gastro-intestinal catarrh, inasmuch as in a large majority of instances a catarrhal condition of the mucous membrane occurs at the commencement of enteric fever, and should be taken as pointing to an impending attack of that disease.

Now, having pointed out the errors of treatment which so often follow an erroneous or incomplete diagnosis, there still remains, in my opinion, another source of danger, and it is this—that the diagnosis having been made correctly, some persons deliberately and advisedly administer powerful purgatives and astringents for the cure of the disease; in the first case with the view of eliminating the specific poison of the disease, and in the second, with the intention of sustaining the patient's strength.

It appears to me to be extremely irrational to expect that in a case where the bowels are already free, possibly too free, that any addition eliminative power will be gained by the administration of a stimulant to an already over-acting organ; and, further, we should not forget that we have an irritated, inflamed, and probably ulcerated intestine which is sadly in want of rest to enable it to return to health. The prescriber of astringents, on the other hand, also forgets the inflamed and ulcerated intestines, and that by the use of astringents he is retaining irritating and decomposing matters in contact with the ulcers, thus increasing their irritation, and promoting the tendency to septic poisoning and deep ulceration, which are the great dangers in enteric fever.

Now having so severely criticised the practice of others, I may be fairly expected to mention what practice I pursue myself, and consequently what I recommend. It is difficult to treat of *one* condition of a disease without considering the treatment of other concurrent states.

In the first place, I consider it essential to the fair progress of a case of enteric fever that the bowels should be more frequently moved than in health, and that the motions should be plentiful. I consider that the bowels may be moved with advantage to the patient four times in twenty-four hours, and should never be allowed to remain confined for more than forty-eight hours, and not so long if any symptoms of distension or pain set in.

The measures I take to promote these objects, in many instances, are confined to mere regulation of diet—and for many valuable hints upon this point I am indebted to my friend and former colleague, Dr. H. Kennedy, who has published an interesting paper on this subject in the Practitioner. Thus, if the bowels are moved more than the required number of times without the diarrhœa being a severe symptom, I find that feeding the patient on boiled milk alone will be sufficient—if not, the addition of saccharated lime-water will probably prove effective. If these remedies do not keep the diarrhœa within reasonable bounds—say under six motions in the twenty-four hours—I employ sulphuric acid in the proportion of ʒ 3 to an ʒ 8 mixture, one ounce to be taken every three hours. This I generally give as an addition to a mixture containing quinine, which I almost invariably employ in large doses in treating enteric fever. Should this fail I add morphia or tincture of opium to the mixture in small quantities. I find that in cases where the boiled milk treatment is employed early little if any astringent medicines are required. I never employ beef-tea when there is a tendency to excessive freedom of the bowels. In cases of extreme diarrhœa I have employed the lead and opium pills of the Pharmacopœia in 4 gr. doses every fourth hour with great benefit. At the same time I employ linseed poultices over the abdomen, and stupes of turpentine or mustard where pain or tenderness is much complained of. The treatment of constipation is a more easy affair. I may say for this purpose I employ a single drug—namely, castor-oil, and usually muzzle it with opium. I seldom give more than a teaspoonful for a dose, and in many cases but half that amount. In the early stage of the disease, when I find the bowels have been confined for some days before the patient came under treatment, I at once give a dose of castor-oil. This not only benefits the patient, but in a doubtful case assists the diagnosis by often producing a characteristic evacuation. Great caution must always be observed in giving meat in early convalescence, as it is likely to produce diarrhœa. I prefer here to begin with chicken-broth, then chicken, and lastly mutton. If a rise in temperatue occurs after a change of diet, diarrhœa may be expected, and should not be waited for; the meat should be at once discontinued, and the milk resumed. In cases of hemorrhage I have found ergot the most useful remedy, and so far have never lost a case of enteric fever by hemorrhage.

I believe the main point to be attended to in the management of the bowels in enteric fever is to keep them *free*, but *not too free*, and to avoid, as much as possible, purgatives or astringents.—*Dublin Journal of Medical Science, Feb.* 7, 1877, *p.* 128.

7.—ON THE USE OF PEROXIDE OF HYDROGEN FOR THE PREVENTION OF THE SPREAD OF SCARLET FEVER AND SMALL POX.

By Dr. JOHN DAY, Geelong, Australia.

In October, 1875, by request of the Mayor of Melbourne, I drew up a report, which was subsequently published by order of the Local Board of Health, on fifty-one cases of scarlet fever which had been treated by me between April, 1873, and April, 1875. These comprised all the cases of scarlet fever which had come under my charge during that period. They were all treated in a similar manner. Each patient was freely rubbed over the whole surface of the body three times a day with a preparation composed of one part of ethereal solution of peroxide of hydrogen (erroneously called ozonic ether) and seven parts of pure lard, well incorporated without the aid of heat. The inunctions were continued for about three weeks. No other remedies were prescribed, except in a few cases where the throat symptoms were severe, when a gargle, composed of two drachms of ozonic ether in eight ounces of water, was ordered to be used every second hour.

These fifty-one cases occurred in thirty-eight different houses, and in four houses only was there any extension of the disease. There were no deaths. Since the above-named period I have attended sixty-four cases, occurring in fifty different houses, and in three houses only was there any extension of the disease after I had commenced my treatment. I have been less fortunate, however, in my results, having had six deaths.

Peroxide of hydrogen contains a larger amount of oxygen than any other known substance, and, moreover, one-half of its oxygen is loosely combined and in a highly active condition, ready to combine with any organic matter with which it may be brought in contact: so that it would appear to be an agent specially suited for the destruction of the poison-germs of scarlet fever, small-pox, and other epidemic diseases. Dr. William Squire, in an excellent paper "On Sanitary Precautions against the Infectious Eruptive Diseases," read before the National Association for the Promotion of Social Science, says—"It [infection] cannot be carried far in the air, for fresh air oxidates and destroys it, so that for the most subtle diseases the infecting distance is small." Now, the loosely combined atom of oxygen in each molecule of peroxide of hydrogen is infinitely more potent as an oxidiser than the oxygen of the atmosphere; consequently I think it is reasonable to infer that by coating the body of a person suffering from scarlet fever or small-pox—diseases in which most of the poison is eliminated by the skin—with peroxide of hydrogen

in combination with lard, cacao butter, cold cream, or any other substance which will conveniently retain it, we are reducing the danger of infection to a minimum. I have recently slightly modified my formula for the external application, and now generally prescribe it as follows:—Ozonic ether, four drachms; pure lard, four ounces; benzoic acid, twenty grains; otto of roses, four drops; to be carefully mixed without the aid of heat. The benzoic acid, in addition to its being a powerful antiseptic, possesses the property of allaying cutaneous irritation, a symptom often very distressing to scarlet fever patients. The otto of roses gives an agreeable odour to the preparation.

I now also prescribe, throughout the whole course of the disease, a mixture composed of two or three drachms of ozonic ether in half a pint of water; the dose ranging from a teaspoonful for a child twelve months old, to a tablespoonful for an adult, to be taken every second hour. This is used for the double purpose of benefiting the throat symptoms and disinfecting the breath.

I have so much faith in the disinfecting properties of peroxide of hydrogen that I recommend all my friends and patients who are in a position to afford it to use freely that which, for want of a better name, I call oxygenated perfumery. It is made by adding ozonic ether, in the proportion of about a drachm to the ounce, to any kind of perfume, according to individual taste. I give the preference either to Rimmel's toilet vinegar or eau de Cologne. Letters, newspapers, and articles of clothing may be disinfected by sprinkling them over with oxygenated eau de Cologne, or with any other oxygenated perfume.

With regard to any power that peroxide of hydrogen may possess of destroying the poison-germs of small-pox, I must confess myself to be merely a theorist, for we have not yet had small-pox in its epidemic form in Australia. In 1871 a vessel arrived at Melbourne with small-pox on board, and shortly after the passengers were landed a few cases broke out in different parts of the colony; but through the energetic measures adopted by Dr. McCrea, our chief Medical Officer, the disease was soon stamped out. At that time, however, it first occurred to me that it might be possible, by a process of oxidation, to destroy the poison-germs of small-pox as rapidly as they were given off from the body, and in a paper "On a Means of Arresting the Spread of Small-pox," read before the Medical Society of Victoria, July, 1871, I suggested the use of peroxide of hydrogen for that purpose.

From a theoretical point of view it might be supposed that peroxide of hydrogen would act more powerfully as a disin-

fectant in small-pox than in scarlet fever, in consequence of the curious property that pus-cells possess of exalting its chemical activity and giving it the oxidising powers of ozone.—*Medical Times and Gazette, March* 10, 1877, *p.* 256.

8.—ON ALCOHOLISM.

By Dr. Samuel Wilks, F.R.S., Physician to Guy's Hospital.

Although alcohol may not be directly a food, yet indirectly it might be so; for, if two men be taken, and one have nothing whatever to eat, and the other have alcohol given him, I presume the latter will remain alive the longer. It must certainly be oxidised; and persons who take a large quantity of spirits grow fat, as we shall presently see.

If, however, we do not understand its physiological workings, yet we can see the effects of it on the system for all practical and clinical purposes. In the first place, does alcohol appear to be a necessary food? There can be but one answer. There are many nations who do not take it, and some whose religion forbids its use. Is it necessary for us? Well, you know many in this country who do not take any. It is not a necessity then; it is for this we have to contend, and, if I can impress this on you, the hour will not be wasted. English people are, however, too often brought up with the idea that it is a necessary article of diet. Patients will take their wines and spirits even when they are doing themselves harm, and, if you object, will ask, "What must they do?" You tell them to do without them; to which they will reply that they must take something. I want you to get it thoroughly out of your minds that there is any *must* in it, and start afresh with the idea of its non-necessity.

Let children always live and grow up without alcohol: in after years, when we pass an artificial life, there may be reasons for taking it; but remember, even then it is not an absolute necessity. Start with this principle; let your patient, even an adult, try to do without it, and then, and if circumstances seem to suggest it, let him have his glass of wine. I do not say that a number of persons can do entirely without any in our present mode of living, but let us regard alcohol in its true light, as a luxury, as we do tea, tobacco, &c. If we do this, we are safe. I cannot recommend you to live entirely by rules and natural laws, and give up all the conventional luxuries of life, for then we should dismiss more than half the dishes from our table. I do not want this to come about, and, for my own part, I like a glass of wine or a cigar as well as other people. There is in to-day's paper an account of some vegetarians who never eat any meat. I do not advise you to follow their example, but it shows you

that meat is not essential to life. We might, I have no doubt, live on what Dr. Johnson states Scotchmen and horses do, viz., oats.

What are the effects of a small dose of alcohol? It is said to be stimulant. If a man be jaded and tired, it gives a sort of temporary support; a little beyond this point, and he is depressed, the stimulant effect lasting only for a time. There is a dilatation of the vessels and warmth of the surface taking place; at the expense, however, of internal heat. In large doses, the temperature goes down. On this point read two cases mentioned by Mr. Carrington, in that admirable essay of his on Alcohol in Guy's Hospital Gazette.

Do these small amounts really stimulate and help one in his work? I ask the sportsman; he says he gets tired, and then has lunch, after which he feels comfortable and jolly, but never shoots another bird. It is the same with billiard-players. A violin player in my house was advised to take a glass of wine for his excessive nervousness, but refused, saying, "I know I shall lose all my nervousness, but I shall also lose my touch, and my notes will be blurred, and I shall be the last to find it out, although it will be very apparent to others."

You see, therefore, it does not stimulate or add edge to our accomplishments; but we might ask, does it add to our strength, or enable us to endure longer? To answer this, I will refer to a little book in my hand by the late Dr. Parkes, entitled, On the Issue of a Spirit Ration during the Ashantee Campaign. This book contains the reports of the medical officers on the effects of spirits doled out to the men. The result is given in the short preface written by Dr. Parkes, to the following effect.

"When, as frequently happens in campaigns, soldiers are marching nearly the whole of the day, and can obtain their regular food only late in the evening, what can be given to lessen the sense of fatigue, and to enable them not only to continue the march, but to be ready for any emergency which may arise? The usual resort is to a spirit ration, and there is no doubt that for a time this exerts a reviving effect. But is it the best thing which can be given, and are its advantages without alloy? I think it can be shown that it is not a perfectly reliable aid, and requires, when used at all, to be so with a full knowledge of its mode of action. The first effect of alcohol when given in a moderate dose (for example, what is equal to one fluid-ounce of absolute alcohol) is reviving; but this effect is transient. As shown in the report, the reviving effect goes off after, at the utmost, two-and-a-half miles of additional march, and sometimes much before this; then the previous languor and sense of exhaustion not only return, but are sometimes more intense, and if alcohol is again resorted to, its effects

now are less satisfactory. Its reviving power is usually not so marked, and its peculiar anæsthetic and narcotising influence can often be distinctly traced. The men feel heavy, dull, disinclined to march, and are less willing and cheerful. It is clear, then, that alcohol is not a very trustworthy aid; for, supposing a commanding officer having marched twelve or fourteen miles, and desiring to cover ten more miles, finds his men weary, and not being able to halt and feed them, orders an issue of spirits of an amount sufficient to revive, but not to depress; the first effect will be good, but, in less than an hour, his men will be as weary as before, or probably more so. If, then, he re-issues the spirit within so short a period of time, it is certain that, in the case of many men—perhaps the majority—the marching power will be lessened. Even the reviving power of the first issue is not always so considerable as might be supposed; and, indeed, I have been surprised to find how little good effect it has sometimes produced."

The fact is that alcohol, as usually taken, is not a stimulant at all. It is a depressant and narcotic. People are simply under a delusion when they think it otherwise. We ought to change its name, and we should then get a proper notion of its character. I believe this change would tend more than any other single circumstance to make people cautious in its imbibition. It is taken for the same reason as chloral, and as opium in other countries. If you regard it as a narcotic, you will then better understand all the consequences of its use. A man in a drunken brawl over night gets his teeth knocked out. The next morning he has no recollection how it occurred, or in what manner he could have met with the accident. Cases such as this are constantly being brought into the police courts, and to some people seem almost incredible.

Alcohol, you see, is an anæsthetic. The man we have just mentioned has felt no pain. In smaller doses, as you all know, it benumbs not only the sense of touch, but that of sight and taste. Every man who has drunk much wine feels that he has lost his taste for the time. He does not know whether he is taking good or bad. "Every man at the beginning doth set forth good wine; and when men have well drunk, then that which is worse." If it were a stimulant, your taste ought to be more refined. It seems to be an utter absurdity to suppose that human nature can crave after a stimulant. For what are people craving? For what is a hard worked man longing? not for a stimulant, but for holiday and repose. It is for repose that every one is seeking. Some miserable people even long for death, "where the weary are at rest." Is not the cry of the lotus-eaters as far reaching as humanity itself: "There is no joy but calm." It is contrary to human nature to crave for

stimulants. The idea is absurd; and the more one knows human nature and its history, the more one wonders how such a name as stimulant could be given to any substance which has had so powerful an influence on the human race as alcohol. It might be known that anything so craved after must be of a soothing, benumbing, or dulling nature. People say they feel better after taking alcohol. Of course they do; one does feel better.

If any of you, whilst working up for your College or Hall, get downhearted, and take a glass of wine or spirits, I have no doubt you feel better; but would you go on with your work? or, would you not go to sleep, or take the newspaper and sit over the fire? If a man have a racking pain in his head, a strong glass of brandy and water will often drive it away: a proof of its narcotising effect on the brain. A man worn out with anxiety and pain, does he want a stimulant to increase these feelings? Is he not making use of a misnomer when he takes a stimulant to drown his sorrows in the bowl? Do not the lower orders, as in an Irish wake, know the benumbing influence on grief? Is it likely they would have recourse to a drink in order to increase their susceptibilities? If it were a stimulant, it would bring out our faculties; but, instead of this, it paralyses our intellect and then allows all the bad passions to have free play. This is the meaning of *in vino veritas*, just as a madman loses his will and control by his higher faculties becoming paralysed.

An immense evil has been perpetuated by giving alcohol a wrong name. It is called a restorative and stimulant; but this it is only to a very slight extent and under special circumstances. Its general effect, and that for which it is almost universally used, is for its benumbing action. I want you to think of it as a depressant, an anæsthetic and narcotic rather than as a stimulant, and you will then get an insight into its injurious effects on the human body.

As a medicine, of course, it is a good one. It is excellent as a sedative. After trying opium and chloral without success, alcohol will often give a good result in the severest neuralgia. It lowers the temperature in febrile conditions, sometimes two or three degrees. This is especially the case in typhoid fever and pneumonia. A quick pulse and high temperature call for it. There was an old man in this state last year in the ward; and I believe his life was saved by the large quantities of brandy that he took. It seems to prevent tissue-change; and large quantities seem to make a person fat. There was one case of it in this hospital some time ago, of a woman who had suddenly taken to drink spirits and became inordinately fat. It is curious that, with all my reluctance to order alcohol unless I clearly see

its necessity, I never find anyone but myself order spirits of wine as a food in order to promote the growth of fat; but its effects in this respect are very striking. Little children wasting away, such as those who are not suckled, may have cod-liver oil and steel wine given them, and yet still waste; but, if put on alcohol, will often get rapidly fat and well. I have now seen several such cases.

What are the effects of alcohol, if taken in excess?

Now, I am not going into the subject of drunkenness; but may mention that some of the effects are possibly due to the impurities put into the spirit. The adulterations of beer, I have no doubt, give rise to other symptoms than those arising from taking the genuine liquor. It is a horrible thing to contemplate that rich people, holding high positions in parliament and society, should be gaining large incomes out of houses where poison is sold. An officer of a regiment met me one day, almost in tears, because one of his men, under the influence of drink, had committed a murder; and, "I believe," he said, "the beer was drugged, and no one is to be punished but the man who drank it." In Paris, there is a terrible liquor called absinthe; and patients are often being brought into the hospitals mad through intoxication from it.

Then, besides ordinary drunkenness, we have dipsomania; a disease for which many want to legislate. The subjects of this are not, for a time, responsible persons. They feel a craving coming on, and sometimes have strength of mind to go at once to a medical man and ask him to take them into his house, or shut them up in a lunatic asylum to restrain them from committing themselves. I once had a clergyman in a country district affected with this under my care; and he had nearly ruined himself. When the fit came on, he used to go to the village ale-house, and take glass after glass until he was drunk. Now, knowing when the fit is approaching, he rushes away from his home and takes the train for London. There is no use in talking to that man; he is as well informed as you; he merely asks for assistance. There is a little book published on this point, styled *Who is to Blame?* It is well worth your while to read it. It is an account of a man who gets drunk, shuts himself up in an asylum, but, as there is no power to retain him, he rushes out when the fit comes on him, goes to the public-house, then home, and kills his wife.

Then, there is chronic alcoholism, bringing about dyspeptic and other symptoms only too well known. I have no hesitation in saying, although I am speaking against the evil effects of alcohol, that a considerable part of my income is derived from the drinking propensities of my patients. Every day some

young man comes to me, with mottled face, yellow eye, and red tongue, saying the first thing in the morning he is sick, and the vomit sometimes streaked with blood; his bowels are loose, and he does not eat his breakfast. I have then heard quite enough to enquire how much whiskey or sherry he takes at 11 a.m. You may have observed that whiskey has taken the place of brandy in the medical dietary. I have failed to discover the reason, so I suppose it is a secret of the distiller's. He, of course, remembers well the ominous hour of eleven; and you then have only one duty to fulfil—*i.e.*, to tell him he is killing himself; and, if that be his object, he had better continue in his course. If not, he must desist; and you will assist him in his endeavour.

If the practice continue, the liver undergoes cirrhosis, and the kidneys become granular; and in some cases there is a special tendency for the cerebro-spinal system to be affected. Thus, in delirium tremens, long before the attack, a man is foolish and half-witted, what is called a good-natured fool. The brain wastes, and weighs several ounces less than it should. This was figuratively expressed by Shakespeare when he said—"Oh, that a man should put an enemy in his head to steal away his brains." The spinal cord also is attacked, and a paraplegia may result, so that the popular saying is quite true, that some persons get drunk in the head and others in the legs. The effect on the head is very well known; that on the spinal cord does not appear to be so readily recognised.

Alcoholic paraplegia is generally found in women of about the middle age of life. It is ushered in by pains in the limbs, then sensation may be partially lost, at the same time some want of power to move them. So you see the chronic action of alcohol resembles much its acute temporary effect where the man getting drunk is narcotised, foolish, and loses sensation, so that one can do anything with him, his hand trembles, he cannot find his house in the street, fumbles in his pocket for his key, and his vision is so indistinct that he declares some one has run away with the key-hole.

Alcohol produces a chronic inflammation of the brain and cord with their membranes. These latter are thickened, and the nerve-centres waste and often become what is called sclerosed. It is very difficult to say when a functional malady has become an organic disease, so that in these cases, however bad they may appear, there is a possibility of ultimate recovery.—*British Medical Journal, Dec.* 30, 1876, *p.* 845.

9.—CASE OF MALIGNANT CHOLERA TREATED BY NITRITE OF AMYL AND HYDRATE OF CHLORAL.

By Dr. Edwin Fairland, Staff Surgeon, Lucknow.

Private J. C., 8th Royal Irish, confined in the Lucknow Military Prison, aged 36, in service fifteen years and nine months, in India one year and eight months, of bilious temperament, and of previous good health, was seized with an attack of malignant cholera at 9.45 a.m. on June 22nd, 1876. In the previous week, there had been two other cases of cholera in the prison, terminating fatally in seven hours each. Both cases were of the most malignant type.

He had suffered for two or three hours from slight fæcal diarrhœa when the symptoms of cholera manifested themselves, and he was at once removed to hospital. The symptoms on admission were constant and severe purging of rice-water motions; very severe cramps in the abdomen and legs, the abdominal muscles being knotted up and feeling like a board under the hand; surface of the body cold and clammy; extremities very cold and livid in appearance; excessive thirst, accompanied by constant vomiting of watery fluid; facies cholerica strongly marked; conjunctivæ flattened; eyes sunken and a dark ring surrounding them; countenance livid and shrunken; breath and tongue cold; voice whispering. He was almost unconscious, being roused with difficulty; and apparently quite callous as to the result of his illness. The respirations were laboured. The pulse at the wrist was barely perceptible and thread-like. He complained of great heat of body, and preferred to lie quite naked. Six hours after admission, he passed about two quarts of rice-water motion. After being under treatment, a slight reaction commenced; the extremities becoming warmer and the pulse slightly fuller—98 per minute; but the cramps and vomiting continued unabated. Prostration was very much marked; he could not raise his head from the pillow. The thirst was intolerable. The symptoms continued the same until twenty-three hours after admission, when he was again purged, passing about a quart of rice-water fluid. The cramps and vomiting were incessant; the temperature unaltered. He dozed off to sleep at intervals. About forty hours after admission, reaction was fairly established; the eyes were becoming fuller and brighter; the complexion lost its lividity; the tongue, breath, and surface became warmer; the pulse fuller, 98; respirations easier, 14 a minute. About fifty hours after the seizure, he complained of pain in the back and limbs and great debility, and became very restless, sinking down to the foot of the bed, and very despondent. Thirst was extreme, and vomiting incessant. The cramps occurred at

long intervals. There was no purging. He was slightly incoherent and wandering in his ideas, though conscious when sharply spoken to; he had occasional muttering and great restlessness. Sixty-nine hours after the seizure, and for the first time since early on the morning of the seizure, he passed twelve ounces of urine. His bladder had been empty up to this time. On the morning of the fourth day, the conjunctivæ became highly inflamed; the skin remained moist and warm; pulse regular and full. He voided about eight ounces of clear and limpid urine. He continued very restless, complaining of headache. At 9 p.m. he passed a large quantity of urine. The purging had completely ceased. He had been sleeping. The thirst continued; the vomiting had ceased. The inflammation of the conjunctivæ was diminishing. There was no headache, his mind was clear; great debility remained. On the sixth day, the bowels were opened by castor-oil, the motions being normal. From this time he continued to improve. The debility was overcome by appropriate nourishment, and, on the tenth day, he was removed to another hospital for change.

Treatment.—Soon after admission, I gave him five minims of nitrite of amyl by inhalation, and continued this steadily every half hour until twenty hours after admission, when it was inhaled hourly for fourteen hours more. It was then administered every two hours in the same doses until fifty-six hours after admission, when it was discontinued altogether, reaction having been well established. Five drachms had been inhaled altogether. I also hypodermically injected hydrate of chloral in two grain doses—strength of solution 1 in 10. Of this drug, sixty-four grains were injected altogether, at intervals of about two hours, in the arms, legs, and twice over the epigastrium. No evil results locally followed any of these injections. About twenty-nine hours after his seizure, the following mixture was taken every two hours: ℞ Liquoris ammoniæ acetatis ʒij; potassæ nitratis gr. xv; spiritûs ætheris nitrosi ʒj; olei juniperi ♏ij; aquæ ℥j. M. After taking this medicine for twenty-eight hours, the kidneys resumed their function, and twelve ounces of urine were passed. His body was frequently sponged over with tepid vinegar and water. Sinapisms were applied over the kidneys, and, on the third day, I allowed him a small quantity of beer, which he retained, and which seemed to promote diuretic action. During the whole time, he was allowed to drink as much soda-water and ice as he needed.

In concluding these notes, I wish to say that I have recorded them for the sake of information; a much more extended experience being necessary to prove whether either or both the two drugs relied on in this case had any real curative power

over the disease that has hitherto proved so unmanageable. I was induced to try the nitrite of amyl by reading some remarks on its action by Dr. B. Richardson; and, last October, a case occurred in the military prison very strongly resembling cholera, in which I tried it with the most satisfactory results; warmth and animation to the collapsed system being almost immediately restored by its use. I have also employed it in a few native cases of cholera with more or less satisfactory results; but it is not easy to employ amyl in such cases and note the results with the same accuracy as in European cases.

I used it, also, in the two cases of cholera referred to in the opening lines of these notes; but the disease had seized the poor fellows with such full force, that nothing short of a perfect antidote to cholera could have saved them.—*British Medical Journal*, *Jan.* 27, 1877, *p.* 102.

10.—THE TREATMENT OF ACUTE RHEUMATISM BY SALICIN.

By Dr. John Curnow, Assistant-Physician to King's College Hospital.

The treatment of acute rheumatism is so varied and unsatisfactory, that any new method or novel therapeutic agent is sure of attracting much attention, and the discovery of any means of arresting the course or diminishing the complications of so common and perilous an affection cannot be too highly appreciated. Dr. Maclagan's success with salicin is so very remarkable that it is incumbent on other physicians to state their experience of this remedy. Whilst in temporary charge of the medical wards at the Seamen's Hospital, I treated four cases of well-marked rheumatic fever on this plan, and I purpose giving a short abstract of these cases and the results. I am indebted to Mr. A. G. Lacy, the acting house-physician, for the notes.

Case 1.—R. W. V., aged twelve, was admitted on May 1st, 1876, with considerable effusion into both knees and ankles, and complaining of great pain and tenderness in those joints and in the hips. He presented the ordinary symptoms of a moderately severe attack of acute rheumatism, and had been ill for six or seven days previously. A systolic bruit was plainly audible over the apex of the heart, but this was probably due to a former attack from which he had suffered four years previously. His temperature was 101° on admission, 101·4° at night, and 102° on the following morning. He was then ordered salicin in ten-grain doses every two hours. Two days after the administration of the remedy his temperature fell to 99°, and the

pain was much less, and in five days from his admission he was practically convalescent.

Case 2.—J. H., aged twenty-three, was admitted on June 19th, 1876, with an attack of much the same severity as the preceding. He had been ill for two days only, and the joints affected were the knees, ankles, and wrists. His temperature on admission was 100·6°; it rose at night to 101·2°, and the next morning and evening was at 101·6° and 102° respectively. From this time he began to improve. His temperature fell to 101° and became normal on the sixth day after admission. Salicin in ten-grain doses every two hours was administered from his admission. There was no cardiac complication, although the heart's action was at first very rapid and irregular, but it soon quieted down. It was his second attack, and the first had lasted for six weeks under treatment in a hospital.

Case 3.—C. B., aged twenty-three, was admitted on June 16th, 1876, with an attack of acute rheumatism affecting his right knee and elbow. His temperature was 102°, and fluctuated between 101° and 102·4° for the next three days, during which period he was taking salicin in fifteen-grain doses every two hours. On the 20th the evening temperature rose to 103°, but from this period the disease gradually subsided, and on the 25th the temperature was normal. There was no cardiac complication.

Case 4.—M. L., aged twenty-two, was admitted on July 8th, with well-marked symptoms of rheumatic fever. He had been ill for six days, and had very extensive effusion into both knee-joints, the left especially. His ankles, wrists, metacarpal and phalangeal joints were red, swollen, and exquisitely painful. On admission his temperature was 101·8°, pulse 108, and respiration 32. At night his temperature was 102°, and on the next morning it had fallen to 100·6°. The apex of the heart was displaced downwards and outwards, and a systolic bruit was heard over it. He had suffered from two attacks previously, one lasting thirteen weeks, and the other four weeks. Salicin was ordered on his admission, in doses of ten grains every two hours. On the 10th the dose of salicin was increased to fifteen grains, and on the 11th to twenty grains every two hours. The swelling in the knee-joints had much diminished, but he had slept badly, and his general condition had not improved. His temperature was fluctuating between 102° and 103°, his pulse had risen to 120, and his respiration to 40. On the 12th he was covered with sudamina, the heart's action was irregular and tumultuous, and on the 13th a distinct pericardial friction was heard. His urine was now alkaline, but it contained no albumen. On the 14th the rub had increased in

intensity, and its loud grating to-and-fro character enabled it to be felt as well as heard, whilst the area of the cardiac dulness was also a little enlarged upwards and laterally. He now complained very much of acute pain in the cardiac region, which was relieved by the application of three leeches. The salicin was still continued; poultices had been applied to the chest from the commencement of the pericarditis, and the joints wrapped in cotton-wool throughout the disease. On the 18th the pain in the chest had entirely ceased, but the cardiac dulness was still exaggerated, and the friction-sound loud and grating. The wrists and fingers were still swollen and tender, but the other joints were free from pain. The salicin was now stopped, a blister was applied over the cardiac region, and six ounces of brandy were administered daily; no other drug was given. On the 21st the rub was less distinct, the temperature, pulse, and respiration began to lessen, and the brandy was diminished. From this time a tedious recovery took place, and on the 28th the temperature became normal, but his convalescence was very prolonged.

The first three cases seemed to indicate that the salicin treatment was the true desideratum in this affection; but in the last case, the absence of any relief whatever, the supervention of a very acute pericarditis, the powerlessness of the remedy against this complication and the attendant fever, must make us hesitate before we finally accept Dr. Maclagan's dictum—"that it (salicin) seems to arrest the course of the malady as effectually as quinine cures an ague or ipecacuanha a dysentery; that, given sufficiently early and in sufficient dose, it prevents these (cardiac) complications; and that its free administration is the best means of staying their progress after they have occurred." No objection can be taken as to the sufficiency of the dose, for the salicin had been so freely administered that the patient had taken over 800 grains in the five days before the friction-sound appeared, and his heart was most carefully examined two or three times daily. I would incidentally point out that the urine became alkaline after the illness had existed for some days; and I have noticed a similar condition in other cases of acute rheumatism even when treated by mineral acids or by expectants. This must make us cautious in any inferences into which we may be led as to the effect of our drugs upon the urine in this disease.—*Lancet*, *Nov.* 18, 1876, *p.* 708.

11.—NOTE ON GOUT.

By Dr. G. Owen Rees, F.R.S., Consulting Physician to Guy's Hospital.

Notwithstanding all that has been written and said con-

cerning gout, we still find our knowledge of its pathology most uncertain, and the views of our modern authors differing in a remarkable manner. Having lately perused a valuable lecture by Sir James Paget on what he terms gouty phlebitis, I have been reminded of many facts and views observed and considered by me for years past, and these have proved of more interest since reading that lecture, and I have become the more inclined to entertain the belief that the phenomena of the acute gouty paroxysm are best explained on the theory that the disease is essentially a capillary phlebitis, the venous inflammation being caused by the circulation of a blood-poison.

The comments made by our best known authors on gout, when alluding to the works of their brother-labourers, show that serious objections exist against the reception of any theory yet propounded in explanation of the acute attack. Thus, while Dr. Garrod believes that the presence of uric acid in the blood constitutes gout, and that the effusion of urate of soda is the proximate cause of the fit, we find, on the other hand, that Dr. Gairdner gives reasons for altogether doubting the correctness of that view, stating that Dr. Garrod's theory "not only does not explain the symptoms, but is absolutely contradicted by them."

When considering the opposing opinions of authors, it has often struck me that there must be some condition necessary to the production of gout other than the presence of urinary constituents in the blood, while it must still be admitted that such contamination plays a considerable part in the causation of the disease. We cannot doubt that, before any attack of gout occurs, the blood always contains an excess of urid acid. This, Dr. Garrod maintains, produces the attack by depositing as urate of soda in the tissues. Dr. Gairdner argues that, if it be so, the inflammatory symptoms ought only to set in when the effusion of material takes place, whereas the severe pain and vascular excitement appear *before* the swelling takes place and subside immediately on its appearing.

These litigants, however, cannot but agree on one point; viz., that there is an excess of urid acid in the blood, though they differ so materially in regard to the effects attributable to its presence.

Now, we know that animal poisons of obscure character are occasionally produced in the blood, and cause inflammation of the veins among other evils. Is it not probable that, in gout, the blood, charged with urinary material, becomes so changed, that a poison is elaborated which inflames the venous capillaries? Sir James Paget has shown that the larger venous trunks are especially prone to disease in gouty persons. If so with the larger trunks, why not so with the capillaries?

The phenomena of the gouty seizure may, I think, be more satisfactorily explained on this theory than upon any other. The disease thus beginning in the elaboration of a poison from the urinary materials found in the blood, the venous capillaries inflame as a consequence; severe pain is experienced, which is relieved immediately when effusion and swelling occur.

Dr. Gairdner's views have especial reference to the veins, but he does not believe in any poisonous influence nor in the phlebitic state. He considers, however, that the "real seat and cause of the painful phenomena of gout" is in the distended venous capillaries, and he accounts for this distension as follows. "This fluid (the venous blood) is compressed between two opposing forces, that, viz., which is derived from the heart and arterial system, urging it forward on its course, and, on the other hand, the antagonistic resistance of the great veins leading to the right auricle." This position is attacked by Dr. Garrod, who states that the general state of vascular plethora is by no means always present, and its absence is observed in spare subjects and in those who strongly inherit the disease. This objection seems well founded.

If we assume, however, that venous inflammation is present, then, of course, we can explain the condition of the larger veins of the limb, which, thus affected by poison, will not be able to propel the venous current, and will become distended and remain so till the poison is eliminated or destroyed.

It seems certain that, when urinary matters are retained in the blood, very different poisons may be formed, owing to some varying causes of which we know but little. Thus, in Bright's disease, the blood becomes so contaminated, but the effects, as is well known, are very different from those of the gouty seizure. The poison produced is of quite a different kind, its most distinctive character being shown in its powerful action on the brain and in the production of convulsive action.

It is, of course, impossible to say how or why it is that the same urinary matters should become converted into different poisons in the blood in different diatheses; but, if we turn to non-vital chemistry, we find analogical results. The ferment present may differ, the fluids may vary in density or in temperature, and, in the one case, a poison may be produced capable of causing violent convulsion, and, in the other, a milder toxic agent may appear as a result, causing inflammation of venous capillaries and all the phenomena of acute gout.

In connection with this subject, I would wish to direct attention to the fact that a condition may be brought about by the gouty poison, which it is difficult to separate from ordinary pyæmia. I lately met with such a case in the person of a gentleman engaged in large practice, whose diathesis was decidedly

gouty. The disease seized him in nearly every joint, and was treated at the onset with the ordinary approved remedies. After a few days had elapsed, he became anxious, and I was called to see him. I found both hands and feet swollen and tender. One knee was involved, and he complained a good deal of the back of his neck. His pulse was extremely weak, and so rapid that I counted it with difficulty. Notwithstanding this state of the circulation, his intellect was quite clear, but his respiration was rapid and his temperature 104 deg. My first idea was that the case was one of ordinary pyæmia; but from the history there could be little doubt that the disease commenced as gout. This gentleman recovered well under the free use of stimulants. During the progress of the case, there was a good deal of tenderness for two or three days over the saphena major vein, but the vessel did not become hardened or thickened. A pain occurred also over the hip during convalescence, and there was considerable tenderness over the part for a day or two. If the view I have here taken of the nature of gout be correct, it would appear that in treatment our object should be to exhibit some remedy capable of acting upon the poison and destroying its virulence. The power which colchicum evinces looks much like this, acting, as it often does, so very rapidly in removing gouty inflammation.

There are one or two points having reference to the treatment of this disease upon which I will venture to remark.

1. Too little attention seems to have been paid to the amount of solid food allowed to gouty patients. Most of them eat too much.

2. It is much the fashion to prescribe strongly alkaline waters in large quantity. This depresses the gouty, and tends to impair the digestive power of the stomach. In moderation, however, the milder alkaline waters are of value.

3. Though it is true in the general that alcoholic drinks in the form of spirits are better than wines, yet it often happens that these, after a time, become very objectionable to the patient, and prevent him eating his food with appetite. In this case, it is always better to allow wine, which should be always of the highest quality. From what I have observed, I am much inclined to believe that really good wines are generally used to advantage, and that the vegetable extractives of these mellowed specimens are easily digestible by the gouty stomach.

4. Much may be done by remedy to ward off the gouty seizure, and the patient generally receives a warning. When such symptoms arise, a combination of quina, colchicum, and colocynth, taken twice or three times a day, is an excellent medicine.

Some few years ago, I saw two gouty cases which much impressed me, and which showed how dangerous it is for gouty

people to take quantities of cold water day by day, even though the quality of the fluid be of mild character. During a stay at Homburg, I was entreated to see an Englishman who was said to be in imminent peril. I found an inveterately gouty man with an immensely distended abdomen. The heart's action was thereby much impeded, and he was nearly pulseless. This was brought about by taking the waters in accordance with the generally prescribed rules. I saw a very similar case shortly afterwards. The treatment consisted in prescribing an opiate at night (for there had been great loss of sleep in both cases), and in administering alcoholic stimulus in the form of warm brandy and water. The patients recovered rapidly.—*British Medical Journal, Feb.*25, 1877, *p.* 225.

12.—ANHIDROTICS.

By Dr. J. Milner Fothergill, Assistant Physician to the City of London Hospital for Diseases of the Chest (Victoria Park).

This is a new term which is rendered necessary by the progress of therapeutics. Anhidrotics are agents which check profuse perspiration. The term hidrosis signifies excessive sweating, and hydrotic is the name of an acid found by Favre in sweat. By the addition of the alpha (α) privative to this adjective, we can form the word "anhidrotic," a useful addition to our nomenclature; as we find that there are several remedial agents which possess the property of checking profuse perspiration.

[Many remedial agents have been and are used for this purpose. Some are administered internally; others applied externally. Cold sponging with vinegar and water. Heat in the form of sponging with water as hot as can be borne. Dilute phosphoric acid internally. Sulphate of copper is a powerful astringent and checks night sweats. The vegetable astringents, tannin and gallic acid, are sometimes of use.]

The most potent of all anhidrotics, in my experience, is unquestionably belladonna. We are indebted to Dr. Sydney Ringer for our knowledge of this property of belladonna: and the debt we owe to him can only be sufficiently estimated by those who have an extensive experience of phthisis, and who give the drug a fair trial. I have no hesitation in saying that the use of this agent completely changes the aspect of many cases of pulmonary phthisis. For the arrest of the exhausting night-perspirations of phthisis, belladonna is as potent as digitalis is in giving tone to a feeble heart. It is quite true that neither is very effective in the last and final stages of

disease, for indeed nothing is very potent then; but in the early stages the action of each is very pronounced. In the night-sweats of spreading caseous pneumonia, the administration of belladonna is followed in almost all cases by a decided arrest of the flux; and in many cases the arrest of this flux is accompanied by immediate improvement. A few of the worst cases only go on entirely unaffected. In the colliquative sweats of the last stage, when the lung is breaking down extensively, the influence exercised is but small; still it usually palliates the drain to some extent even then. The loss of the salts of the body in profuse perspiration quickly exhausts the system; and the arrest of this drain commonly permits of the other measures being effective in improving the general condition. While the loss goes on unchecked improvement is impossible.

To produce these effects it is necessary, however, to use larger doses than those spoken of by Dr. Ringer. He speaks of from $\frac{1}{200}$th to $\frac{1}{100}$th of a grain of atropine given hypodermically, and of from $\frac{1}{80}$th to $\frac{1}{40}$th, by the mouth. I have had no opportunity of trying the hypodermic method; but as to the doses given by the mouth, I usually commence with $\frac{1}{75}$th of a grain and go up to $\frac{1}{25}$th; the latter dose rarely failing. I am inclined to think that in Mr. William Murrel's sixty cases referred to by Dr. Ringer the large proportion of failures (from 8 to 10 per cent.) was due to some extent to his not pushing the drug. When $\frac{1}{75}$th is ineffective, I prescribe $\frac{1}{50}$; if next week that has failed, $\frac{1}{25}$th is ordered. This usually produces the desired effect, after which smaller doses will maintain it, and may be continued. For instance, in one case at Victoria Park Hospital on July 22nd, $\frac{1}{50}$th was ordered; the patient at the same time taking a mixture of iron and strychnia, with ℈ii. of sulphate of magnesia, three times a day. This did well for a week or two, when the night-sweats returned, so that on August 19th the dose was increased to $\frac{1}{25}$th. The effect of this was pronounced, and on the 26th it was reduced to $\frac{1}{50}$th again; and on September 9th to $\frac{1}{75}$th, which dose keeps the sweats down satisfactorily.

As to the number of cases I have treated by belladonna, I have kept no account; but during the week, July 16th to the 25th, this year, an intensely hot week, 74 patients, out of a total of 300, were taking belladonna at bedtime at Victoria Park Hospital alone. At the West London Hospital I had at least thirty more during the same week. Thus I had 100 at one time under the influence of belladonna. Consequently my experience of the use of belladonna in the treatment of hidrosis is not a very limited one. It enables me to say that belladonna or atropine may be freely used without apprehensions as to any serious toxic effects appearing. Even with $\frac{1}{25}$th of a grain of

atropine every night, the patients do not complain much: some dryness of the throat and a little indistinctness of vision being all: while all prefer these to their dreaded sweats. These effects wear off in a day or two after the drug is discontinued, or even the dose reduced. I have not yet seen any alarming symptoms produced. This I attribute to the gradual increase of the dose; and I have little doubt that if $\frac{1}{25}$th were given at first, many cases would show marked toxic symptoms. But where there seems a tolerance of the drug, the dose must be increased; and may safely be increased. Belladonna is an agent which produces marked toxic symptoms long before a fatal dose is reached; much the same as is the case with strychnia. It is not a treacherous drug by any means: and may be used with confidence. Dr. Charles Kelly (Practitioner, March, 1873) found that in the treatment of hooping-cough, half an ounce of the tincture in twenty-four hours could be safely taken by children of three or four years of age. Without advocating such large doses, until a further experience demonstrates their safe use, I may say that from $\frac{1}{75}$th to $\frac{1}{25}$th of a grain of atropine, and from 20 to 35 minims of the tincture of belladonna are quite safe doses. The atropine may be given in pill; while the tincture of belladonna is best combined with dilute phosphoric or sulphuric acid (℥xv.), and may taken at bed-time; or when the patient wakens about two or three in the morning. It is my intention to try larger doses for the relief of the colliquative sweats of advanced phthisis. As to the actual facts of toxic symptoms of the seventy-four cases mentioned, one had some dryness of the throat; a second had some derangement of the pupils; and a third some indistinctness of vision on getting out of bed in the morning, which quickly wore off.

If any doubts existed as to the causal associations betwixt the administration of the belladonna and the arrest of the hidrosis, they are dissipated by the fact that on omitting the medicine the perspirations returned—as when the patients neglect to attend the hospital, and so are without their medicine. On again taking the medicine the sweats disappear. This puts the matter beyond doubt or cavil, especially when combined with Ringer's experiments, which are well worth perusal.

A few words now as to the practical use of belladonna in the treatment of phthisis. The most common cases are those where a slowly-spreading caseous pneumonia involves one lung to the second, third, fourth or fifth rib. There is a fast pulse, over 100, a temperature over 100° Fahr., cough, profuse night-sweats, and rapid wasting. It is in these cases that the utility of belladonna is so well seen. As soon as the profuse night-sweats are checked, the patient begins to pick up; the appetite

returns; food is better assimilated; the sleep is refreshing; and the mind is much relieved. In fact the arrest of the drain of salts by the hidrosis at once inaugurates an improvement; and the good effects of the other measures resorted to are not lost, as before. It is well at the same time to give the patient tonics, iron with strychnine or quinine, together with mineral acids; good food in liberal supplies, and cod-liver oil if the stomach will carry it. The association of night-sweats with debility is notorious. Fuller recommends some alcohol and food to be taken at bedtime invariably. When the morning sleep is deep the sweats are most profuse, and are "to be in part avoided by keeping awake, which is often done purposely." (Marshall Hall.)

Finally, my experience of pulmonary phthisis is not depressing, but rather encouraging, especially in its early stages. It has been much more cheering since I have employed belladonna extensively. In some cases where the belladonna does not act as potently as usual, oxide of zinc with hyoscyamus is found to be effective. In those cases where the cough at nights prevents sleep, opium may be given with belladonna. The belladonna prevents too great action on the sudoriparous glands, and the combination is very effective. To prevent too much action in the intestinal canal, it is well to give the neurotics in pill with aloes. A pill of morphia ($\frac{1}{2}$ a grain), atropine $\frac{1}{30}$th in three grains of pil. al. et myrrh., is used by me at Victoria Park, and acts satisfactorily. It is not always an easy matter to avoid the undesirable effects of therapeutic agents; and when they must be resorted to, it becomes necessary to provide against and ward off these effects by suitable additions and combinations. There are no serious drawbacks to the use of belladonna, and the dry throat and indistinctness of vision are usually borne by the patients without complaint.

The arrest of the profuse and exhausting night-sweats is usually followed by more or less immediate improvement; and belladonna very rarely fails to achieve this arrest. The systematic use of anhidrotics must grow with further acquaintance with them, and especially with belladonna; and the public, as well as the profession, are under a deep debt to Dr. Ringer, which I trust this paper will do something towards demonstrating. Belladonna seems to be a specific anhidrotic, acting on the sudoriparous glands as it does on the sub-maxillary gland. Heidenhain (Pflüger's Archiv., vol. v. p. 40) indicates that belladonna may be found to affect other glands than the submaxillary by acting on their secreting nerves. Such seems to be its action in the arrest of hidrosis; which it affects when applied locally as well as when given by the mouth or injected hypodermically.—*Practitioner, Dec.* 1876, *p.* 409.

13.—A COLD AND ITS CURE.

By Dr. JUKES STYRAP, Physician Extraordinary to the Salop Infirmary.

It has been well remarked by Dr. George Johnson, that "a cold," or ordinary catarrh, although of itself not a dangerous or serious malady, is nevertheless, with many persons, an oft-recurring one, causing much annoyance and discomfort both to the sufferer and to his associates—of which fact, all of us are doubtless more or less disagreeably cognisant from personal experience; and, as *medical* treatment, notwithstanding popular prejudice to the contrary, has very considerable influence on the progress of the disorder, it is, I think, well worth our while to give the question thoughtful consideration.

The exciting cause and symptoms of catarrh, together with its popular domestic treatment, are too well known to need recapitulation. I purpose, therefore, to limit my remarks to the medical treatment which, for a period of twenty years, I have adopted with considerable success. At the same time, I think it well to note that the treatment refers to that particular form of "cold" characterised by excessive defluxion from the nares and lacrymation, and more or less febrile disturbance (and not to that which, in ordinary language, is styled "a dry and stuffy cold"), and is based on the principle of restoring the natural functions of the skin, which a chilling wind or other atmospheric influence on persons with lowered vitality has wholly or partially suppressed. There are two simple modes of accomplishing the wished-for effect: firstly, by the direct application of heat to the surface of the body by immersion in a warm bath of 100 deg., increased to 110 deg. of Fahrenheit—but in a far more efficient degree by the use of a hot-air bath; and secondly, by the action of certain diaphoretic medicines in combination—which latter are generally sufficient (and certainly the least inconvenient) to effect a cure of ordinary catarrh. In my own person, indeed, I have never found it necessary to have recourse to a bath; still, in severe colds, it may be judicious to combine the two—the bath and the medicine.

My remedy is a very simple one, and the treatment based on the principle recommended by Dr. George Johnson in his recent Lecture on the Treatment of Catarrh and Bronchitis, and which I have carried out for upwards of twenty years with much success.

The difference in our respective treatment by opium, however, would seem to be, that he prescribes it in a "full dose" at bedtime (hence the nausea, headache, &c., to which he refers), with or without ipecacuanha; whereas I invariably give *small doses of morphia and antimony* every three or four hours until

the sneezing and defluxion cease, which, with ordinary precaution, results after the third or fourth dose. The antimony has, in my opinion, a more special effect on the mucous membrane of the breath-passages than ipecacuanha.

The following are the forms which, slightly varied, I have used for many years: a dose or two of either of which has enabled me on various occasions, when suffering from catarrh, to attend to my professional duties with comparative impunity. Confinement, however, to the house for a day or two should, I need scarcely remark, be insisted on, whenever practicable. The warm or hot air-bath (or "packing"), as suggested by Dr. G. Johnson, is a valuable adjuvant to the treatment, if had recourse to on the day of seizure; and, in severe cases, I generally recommend one or the other, if obtainable, and an immediate retirement to bed in a warm room.

℞. Liq. morphiæ (*P. B.*) ℥xl; vini antimon. ℥xxx: potassæ citratis ʒiv; syr. aurantii ʒiij; aquæ ad ℥iv. Misce et fiat mistura, cujus sumat cochlearia magna ij quâqua tertiâ vel quartâ horâ.

℞. Liq. morphiæ ℥xl; vin. antimon. ℥xxx; liq. ammon. citrat. ℥j; potassæ citratis ʒiv; sp. chloroformi ʒj; aquæ ad ℥iv. M. Ft. mist., cujus capiat cochlearia magna ii quâqua tertiâ vel quartâ horâ.

My attention was originally directed to the value of small doses of morphia in catarrh under the following circumstances. Many years ago, I was confined to my room by a very severe catarrhal attack and bronchitis, for which antimony, &c., were prescribed by a friend with but trifling relief. For some reason or other, I was induced to add the twelfth part of a grain of morphia to a dose I was about to take, and in half an hour or so the sneezing and defluxion had considerably abated. The next few doses were taken without the morphia, and the coryza, &c., returned, and the cough became troublesome; in consequence of which, I repeated the morphia, and again the sneezing, &c., ceased. In every subsequent attack of catarrh (to which I was, at one period, very subject), I combined the antimony with morphia; and, having tested their value on myself, prescribed them for others with a like satisfactory result. In what way the morphia effects the speedy relief from discomfort which almost invariably follows its administration, I am not prepared to say. Probably, as Dr. G. Johnson suggests, it is due to some direct influence on the nerves and vessels of the inflamed mucous membrane, rather than to any diaphoretic action. Be that as it may, I would strongly advise such of you as are subject to "colds" just to try the medicine; and I entertain little doubt that the effect of its first trial will be

such as to induce you eventually to thank me for the suggestion of so simple a remedy.

The antimony, in addition to its special effect on the inflamed mucous membrane, tends to counteract the usual constipating action of the morphia; and the citrate or bicarbonate of potass relieves the thirst and itching not infrequently produced (in my own case at least) by the opiate. I would also remark that, by giving the morphia in small and repeated doses of one-twelfth of a grain, combined with correspondingly small doses of antimony, it can be safely administered to persons otherwise intolerant of opiates, without suffering from the headache, nausea, and other distressing symptoms which so often follow a full dose of opium.

Mayhap some will mentally exclaim, "Oh! the principle of treatment has been known from time immemorial." Possibly so. Nevertheless, simple and efficacious as the treatment by morphia and antimony in small doses really is, I can truly assert that, during the not few years in which I have been in the profession, I have never seen it alluded to in any work on medicine, or practised by others than myself; which fact will, I trust, be deemed a sufficient apology for soliciting your attention to it.

In regard to the hot-air bath, I need scarcely remind you that such may be readily extemporised—the chief essential being a capacious spirit-lamp, with a large wick, usually kept in stock for the purpose by surgical instrument makers; and, being made of tin the cost is trifling. The following will be found a simple and effective plan. Let the patient be seated, undressed, in a suitable armchair in his bedroom, and carefully enwrapped in two or three folds of blankets extending from above the shoulders to the floor, but *outside* the chair (or, still better, a hoop affixed thereto), so as to allow a free circulation of the hot air round the body. A Mackintosh cape thrown over the blankets will enhance the effect. The best position for the lamp, according to Dr. G. Johnson, is, with due precautions, between the legs, rather than underneath the chair; and it should be kept burning for twenty or thirty minutes, or until free perspiration be established. The patient should get into a warm bed between the blankets. Nervous people are apt to object to a hot-air bath so constructed, from an absurd fear of the flame of the lamp. The difficulty may be obviated by placing a wire guard over it.

In the absence of the means for providing a hot air or water bath, an effective action of the skin may be induced by wrapping the patient in a sheet or thin blanket (to which latter patients offer less objection than to a wet sheet, on account of the relative warmth-imparting feel), wrung out of moderately

hot water, and enveloping him in a couple of warm dry blankets; in other words, "packing" him, as it is termed, for an hour or more, until free perspiration takes place: a plan of treatment which, I venture to affirm, you will find highly beneficial in renal and other forms of disease.—*British Medical Journal, Dec.* 9, 1876, *p.* 747.

14.—A COLD AND ITS CURE.

By Dr. W. J. Haram Wood, Boston, Lincolnshire.

It was with great pleasure that I read an article in the Journal of December 9th headed "A Cold and its Cure," by Dr. Jukes Styrap, as I have a personal and peculiar interest in this subject, having been considerably plagued—and that frequently—with coryza. The antimony and morphia treatment was not unknown to me; it is clearly laid down in Sir T. Watson's Lectures, and is undoubtedly very good—perhaps the best that can be followed when dealing with private patients, who can be nursed in the house if necessary. I have several times tried it myself—certainly with some relief; but I cannot say that I have found it to shorten or cure the attack, though I am far from denying that it may do so in many individuals. It is, however, a method peculiarly unfitted for medical men and others who cannot remain indoors for a few days, and perhaps would not if they could; and its chief weakness is shown in Dr. Styrap's words, "Confinement to the house for a day or two should be insisted on whenever practicable." Exposed as we are to night-work and sudden changes of temperature, it will not do to be sipping morphia and antimony. What we really require is a method of cure sure and speedy, during which our usual daily work and exposure can be gone through without incurring fresh danger; and such a desideratum has certainly been described by Dr. C. J. B. Williams. It may be widely known, but I fear it is not so extensively appreciated and practised as it deserves. It has been called the *dry plan*, and consists simply in abstinence from drink of any kind until a cure is effected. The theory upon which it is based cannot be better expressed than in the words of Sir T. Watson. "The principle here concerned is that of cutting off the supply of watery materials to the blood. The wants of the system exhaust from the circulating fluid all that can be spared for the sustentation of the tissues, or for the natural evacuations, and there is nothing left to feed the unnatural secretion from the inflamed mucous membrane. Its capillary vessels cease to be congested; the morbid flux is diverted, and the inflammation starved away."

For several years I have made personal trial of this method,

and have the highest opinion of its efficacy. The only drawback is the great thirst, which is at times distressing; but this is felt more, I believe, during the first twenty-four hours than subsequently, when one seems to become accustomed to it; and really it is as nothing, compared with the advantages of a diminishing defluxion, a freedom to follow one's occupation, and a certainty (I believe) of cure. But the chief advantage of the *dry method* has yet to be stated, and that is, it almost never leaves even a trace of bronchial irritation or cough of any kind; and I think this result is rarely attained by any other plan, especially where there is a tendency to anything of the sort.

Good as the method is, I think it may be made more complete and satisfactory by observing the following simple directions.

1. Begin with a sharp aperient in the solid form of pills, swallowing them with as little water as possible.

2. The food should be rather less in quantity and more digestible than usual, and at first should be dry; later on, the moister forms of food are more easily swallowed and digested.

3. As much exercise as possible should be taken in warm clothing, to promote the action of skin and bowels.

The thirst often keeps one awake at night; but this may be prevented by taking a small opiate or fifteen minims of chlorodyne, if necessary; and it is probable that the cold is also benefited by such a dose, but with this difference, that, when the dry treatment has been carried so far as to produce great thirst, it is almost certain that opium will not produce diaphoresis, and therefore does not render exposure more dangerous.

How long must the abstinence from drink be continued? No fixed number of hours will apply to every case. "Until a cure is effected" is a good indication whereby to be guided. Imagine a sharp diarrhœa setting in, and probably the time required would be much shorter; and that is my reason for adding an aperient to the treatment.

As a rule, I think it will be found that twenty-four hours give immense relief; thirty-six or forty-eight hours effect a cure; and sixty hours make it sure and certain. But, during the latter part of the time, I do not think the treatment is at all interfered with by a few teaspoonfuls of water taken with food; and in all cases the return to drink must be very gradual, commencing with small quantities of fluid, which, I need scarcely say, must be simple and non-stimulating.—*British Med. Journal, Dec.* 23, 1876, *p.* 818.

15.—ON THE PROBABLE RELATIONSHIP OF SYPHILIS, SCROFULA, TUBERCLE, CANCER, AND OTHER ALLIED MORBID CONDITIONS.

By Dr. Edward T. Tibbits, Physician to the Bradford Infirmary.

Considering the extent and importance of the subject, it will be anticipated that what I have to say must of necessity be of a very fragmentary nature. And although it is impossible to prove the connexion, of which there is a certain amount of evidence, it is a subject pregnant with material for thought, speculation, and it may be, discovery.

Without attempting in the slightest degree to dogmatise, I would venture to suggest that syphilis has a substantial share in the production of these morbid conditions. Whatever may be the actual relationship, it is quite certain that, to say the least, they have many points of resemblance. If we study their pathology carefully, I think it may be fairly stated that, in all, some portion of the lymphatic system is greatly at fault. And here it will be well to consider briefly the arrangement and distribution of this system. It includes the lymphatic glands, follicles, vessels, capillaries, and "serous canaliculi" described by Recklinghausen. These lymphatics pervade the whole body, being more numerous in those localities where there is the largest quantity of nutritive fluid circulating, consequently where there is the greatest functional activity. They are especially abundant in—(*a*) the mucous membranes, particularly about the lips, mouth, nose, glans penis, anus, and vagina; (*b*) the visceral layer of serous membranes; (*c*) the lungs and liver; (*d*) the ductless glands, comprising the spleen, thyroid, thymus, and supra-renal capsules; (*e*) the vessels of the brain and cord, which are surrounded by lymphatic sheaths.

In the first place we will compare *syphilis* and *scrofula*. Without enumerating the various conditions found in either state, do they not attack almost identically the same portions of the body? Hydrocephalus often appears with hereditary syphilis. Sometimes hydrocephalus attacks one member of the family and syphilis another. Rosen mentions a case of hereditary syphilis in a girl, eleven years old, who had suppuration of glands and caries of bones. Many foreign authorities of high standing in the profession do not hesitate to pronounce many scrofulous diseases in children as the result of syphilis in the parents—e.g., Hey, Hufeland, Mahon, Bertin, &c.; and it is very probable that observations in this direction are conducted with greater facility abroad than in this country. Baumés relates instances in which syphilis in the parents was followed by hydrocephalus, caries about the knee joint, phthisis,

and glandular swellings in different members of the family. Syphilitic parents have been known to produce children some of whom were syphilitic, others scrofulous, others healthy.

In both diseases, whatever be the explanation, there is a tendency to phthisis, although much stronger in scrofula. From comparatively recent investigations made on animals, by Drs. Burdon-Sanderson and Wilson Fox, it appears that a "something," indistinguishable histologically from what is generally understood as "tubercle" can be produced artificially by irritation of the lymphatics. And so in syphilis, when there has been undoubted irritation of the lymphatics, it is not uncommon to meet with a very similar condition in various parts of the body.

Glandular enlargement used to be considered pathognomonic of scrofula (before so much attention was bestowed on lymphadenoma); and yet Dr. Wilks says, "In the lymphatic glands, especially those of the neck, we have now long been in the habit of looking for signs of constitutional syphilis." Mr. Hutchinson says, "Syphilis, in all its stages, produces special and wholly peculiar lesions; and although these may *easily* be mistaken for struma, they have in reality nothing whatever to do with that state of constitution." I believe they may *very easily* be mistaken, and in some cases the diagnosis is decided by the result of our inquiries into the history of the patient. If there is any suspicion of syphilis, we have to do with a syphilitic affection; if not, a scrofulous one. It must be remembered that although pemphigus is peculiarly a syphilitic disease when occurring in infancy, it certainly does not always present the colour which usually belongs to syphilitic eruptions.

Another mark of syphilitic eruptions which does not always hold good is, that they do not itch. I have now under my care two indubitable cases of syphilis in which there is great itching present. Dr. Wilks relates the case of a boy whom he considered scrofulous; but after some time he treated him with mercurial inunction and iodide of potassium, and because he rapidly recovered the case was regarded as syphilitic. Here it was quite certain that the lesion would answer to the description of either disease, the diagnosis being ultimately settled by that most fallacious of all tests, treatment. It cannot be said that the connexion between syphilis, lupus, keratitis, and scrofula can be altogether ignored at present, until we have more numerous and reliable statistics regarding the history of such cases.

Although Mr. Hutchinson and others see in syphilis a great resemblance to the exanthemata, it appears to me that it has many more points of resemblance with scrofula as regards lesions, pathology, and treatment.

Secondly. Is there any intimate relationship between syphilis and cancer? I have seen two or three cases of extensive ulceration about the female genitals. One was syphilitic, and I believe another was epitheliomatous; I do not remember the third. As far as outward appearance went, there was no notable difference, but I did not examine by the aid of the microscope. Out of this arises the following question—viz., Is it possible in every case, at any stage of the disease, to distinguish syphilitic ulceration, rodent ulcer, and epithelioma, in certain situations? Although Mr. Erichsen does not suggest that syphilis has anything to do with the production of cancer, he says that he knows of no greater resemblance than that which syphilis bears to cancer. We have no direct evidence of syphilis being the cause of cancer; at the same time, both these diseases are closely allied to other members of this group, and hence probably to one another.

Thirdly. Many now grant that "tubercle"—i.e., lymphatic overgrowth—is only a variety of scrofula. Wilks considers it a secondary form of scrofula.

Fourthly. Cancer and tubercle appear to be nearly related, for the following reasons:—(1) They not unfrequently attack members of the same family, but not the same individuals. (2) Cancerous and tubercular peritonitis are in some cases indistinguishable; so it is with cancer and tubercle of the lymphatic glands. (3) The rare event (related by Paget in his Lectures on Surgical Pathology) of arrest and almost complete recovery from scirrhus of the mamma connected with the evolution of tuberculous disease. De Morgan considers this indicative of an antagonism between the two diseases. Mr. Bryant mentions a case in which he removed a cancerous breast; the patient recovered with a healthy cicatrix, but died eighteen months afterwards of phthisis. The supposed antagonism between tubercle and cancer may be simply a kind of counter-irritant or derivative action, so that scirrhus of the breast might arrest in this manner the tuberculising process of lung-tissue in its vicinity, or *vice versa*. Certain it is, in many cases of phthisis, glandular enlargement appears to act as a safety-valve, and thus suppresses, retards, or diminishes the activity of the pulmonary disease. (4) Sir James Paget describes some tumours as mixtures of cancerous and tubercular masses, and he affirms that the microscope will ensure a diagnosis, which appears somewhat doubtful in the present day.

Fifthly. Primary cancer of the lymphatic glands is not to be distinguished from lymphadenomatous glands. Wilks calls it lymphoid cancer. *Lymphadenoma* in its typical form is said to be general hypertrophy of the lymphatic glands, accompanied usually with enlargement of liver and spleen. Sometimes both

are enlarged, sometimes one, sometimes the other, sometimes neither the one nor the other. Trousseau says that in three only out of eleven cases which came under his care was there *any* enlargement of liver or spleen. *Leucocythæmia* is probably the same disease, for, like the former, it may be accompanied with splenic enlargement, when it is called "splenic," or with lymphatic glandular enlargement, when it is called "lymphatic leucocythæmia." On referring to the variously-named cases detailed at the Clinical Society of London, on the 24th ult., it appears to me that they differed only in name. I would merely call attention to one or two points. In Dr. Gowers' case we have albuminous urine, hard fibrous or caseating glands, lardaceous degeneration of spleen and caseous infarctions, general lymphatic overgrowth. Dr. Greenfield relates a case of lymphadenoma with an increase in number of white corpuscles; at the same time he says the number of corpuscles (pale) is not large enough for true leucocythæmia. And, further, he thinks this case is not true lymphadenoma; hence he recommends another name for this intermediate disease—viz., "anæmia lymphatica." Dr. Goodhart details a case which he suggests may be lymphadenoma, or leucocythæmia splenica or a mixture of both diseases. Is it not natural to inquire whether there is necessity for so many names in cases so similar? Is the diagnosis of leucocythæmia to rest on the *number* of pale corpuscles in the blood? I would ask, What is the proportion of pale corpuscles to red ones in a normal state? Some authors say one to forty, others one to four hundred. If there be such a wide difference in what is considered healthy blood, it would be desirable to know the proportion of the pale to the red ones in typical leucocythæmia. It is no longer typhoid fever because the rose-coloured spots are too numerous, not numerous enough, or altogether absent? Certainly, if new diseases are to be formed on such data as these, a fresh list might be issued daily from all parts of the country. *Addison's disease* is very like the diseases just mentioned, especially as it is frequently connected with tuberculous degeneration of the supra-renal capsules, which are very like lymphatic glands, and abundantly supplied with lymphatics.

Sixthly. And, further, *tubercle* and *lymphadenoma* must be very intimately associated. In most of Dr. Hodgkin's cases related in the Medical and Chirurgical Society's Transactions for 1832 there was tubercle in lungs, liver, or spleen, and in some of the enlarged glands caseous degeneration had commenced, which Dr. Hodgkin considered a mere coincidence. Caseous degeneration of an enlarged gland has been regarded as a positive proof of the scrofulous nature of the disease, but really it seems to me to be a mark of the great and intimate

relationship between scrofula, tubercle, leucocythæmia, and lymphadenoma. Trousseau says that many cases of lymphadenoma seem to arise from, or at any rate follow, long continued irritation—e.g., ozæna, &c. It is well known that albuminoid infiltration of viscera follows long continued irritating discharges. I have at the present time under my care in the Bradford Infirmary a girl who has had hip-joint disease for eight years, and who now has immense enlargement of the spleen and liver, great excess of pale corpuscles in the blood, and albuminous urine. Moreover she has had hæmoptysis and diarrhœa. In the twentieth volume of the Pathological Transactions Dr. Murchison records a case of a man having a phthisical history, in whom, after death, were found enlarged glands, great thickening of the submucous coat of the duodenum ($1\frac{1}{2}$ in.) tuberculous bodies in the liver and kidneys, and peritonitis. The Committee on Morbid Growths did not venture to give a name to this state of things. In the same volume Dr. Moxon reports a case of lympho-sarcoma of the cervical glands associated with tubercles in the pleura, and he is obliged to confess that the change was closely allied to, if not identical with, scrofula.

Seventhly. In syphilis, scrofula, tubercle, and rickets, it is very common to meet with what is known as lardaceous or albuminoid degeneration of the viscera; and sometimes this occurs in leucocythæmia. We are told by Dr. Wilks that the jelly-like translucent material met with in this condition is very nearly allied to that found in colloid cancer, if not in some instances identical with it. Oppolzer calls albuminoid liver colloid liver. Moreover, scrofula and rickets frequently occur in the children of parents who are scrofulous, syphilitic, cancerous, or tuberculous. However, the same morbid conditions sometimes become apparent in the children of very aged or too nearly related parents.—*Lancet, Dec.* 23, 1876, *p.* 890.

16.—A SPECIFIC FOR PTYALISM.

By Dr. Jukes Styrap, Physician Extraordinary to the Salop Infirmary.

In a very annoying case some twenty-six years ago, after vainly trying all the well-known remedies, I decided on giving sulphur, it having occurred to my mind that "Plummer's pill" (then so-called, and oft prescribed), containing one grain in five of calomel, was seldom known to produce salivation; which fact I also remembered to have heard an old medical teacher attribute to the sulphur in the sulphurated antimony then known as the oxysulphuret. Success, however, did not crown my efforts until, by careful observation, I learnt the proper

mode of administering it, which is in *small and repeated* doses, *special care being taken to diminish the quantity if relaxation of the bowels supervene;* for its peculiar action in controlling ptyalism depends upon its being retained in the system, and not allowed to pass off by the bowels—which, if necessary, should be prevented by the addition of a few minims of liquor morphiæ or tinctura opii. The bowels should not be moved more than once or twice in twenty-four hours. If persevered in regularly every three or four hours, the secretion of saliva and soreness of the gums become very sensibly diminished in the course of thirty-six hours or less; and I have invariably found that its antidotal action is ushered in (or "out," correctly speaking) by the exit of a most offensive gas *per anum*—a fact which you may readily ascertain by inquiring whether, when the bowels have been moved, the evacuations are particularly offensive. The reply I have commonly received has been, "*Very.*"

I do not attempt to explain its *modus operandi*—whether by chemical combination or otherwise. All I can say is that, in the several instances in which I have prescribed it (once in the case of an old military officer aged 70, and formerly an M.D. of Cambridge, who, relying on his whilom medical education, prescribed for and salivated himself), the controlling action was indisputable.

I have generally found that patients suffering from salivation are loth to admit, even when very evident to the medical attendant, that the flow of saliva or soreness of the gums has abated; indeed, they never appear to recognise the relative degrees of soreness, &c., until their attention is pointedly called to the fact that they speak with greater facility; and then, on inquiry, I have usually found that a successful attempt has been made to swallow a little "soaked" bread, and that fewer handkerchiefs are required for the reception of the saliva. In soliciting your attention to the form in which I have been accustomed to prescribe it—

℞. Sulphur. præcip. ℈ij ad ℈iv.; potassæ chlorat. ℈ij. ad ʒ j.; liq. morphiæ ʒ j ad ʒ iss; mist. amygdalæ ℥ viij. Misce bene et fiat mist., cujus sumantur cochlearia magna ij quâqua tertiâ vel quartâ horâ, phialâ agitatâ,—

I venture to express my belief that the antidotal action of the sulphur is entirely independent of, though possibly assisted by, the other remedies. With regard to the potass, I at first prescribed the nitrate, subsequently the bicarbonate, and lastly the chlorate, which, being a neutral salt, does not produce the painful smarting of the gums which the two former preparations do. The mistura amygdalæ, in addition to somewhat disguising the nature of the remedy, offers a bland vehicle for its administration.—*British Med. Journal, Dec.* 2, 1876, *p.* 711.

17.—ON SMALL-POX.

By Dr. Robert Bell, Physician to the Glasgow Ophthalmic Institution.

It is only natural to conclude that the great exhaustion which ensues in small-pox is due to the fact that the highly nervous and important as well as extensive organ, as the skin most certainly is, is in a state not only of great and intense irritation, but of almost complete inactivity as well. Now, the greatest danger of a fatal issue is generally contemporaneous with the development of the suppurative or secondary fever; and, as my method of treating the disease does away with any secondary fever, the greatest, or at least one of the greatest, sources of danger is removed. By commencing this treatment at the very beginning of the attack, the comfort of the patient is secured at once; the skin is rendered less irritable; the fever, in consequence, is kept down; the strength of the patient remains unimpaired; he is able to sleep and take nourishment, and, in short, to pass through the whole course of the attack with the minimum of discomfort. At the period when suppuration commences in the vesicles, and when otherwise a new phase of the disease would present itself, no such unhappiness is encountered, and the patient sails pleasantly through a sea of troubles, quite unconscious that he is doing so. This excellent result is due, doubtless, to the sedative effects of the remedy employed.

The plan of treatment consists in painting every part of the skin where the eruption appears with one part of carbolic acid dissolved in from eleven to fifteen parts of glycerine, and repeating the application night and morning. The urine must be watched with great care, as it often happens that the carbolic acid becomes absorbed and makes its presence known in the urine by giving the fluid a dark smoky appearance. If this be observed, the application must be made less frequently, or a weaker solution of the acid employed, as it may act too severely as a depressing agent, though I never knew this actually to result from the use of even the more concentrated solution. The employment of carbolic acid in this way has other advantages besides those already mentioned. It acts as a disinfectant, and it prevents pitting to a very great extent. The latter effect is due to the fact that suppuration does not run the same lengthened course as it does when no carbolic acid is employed, and thus the skin is not destroyed to such a depth as it would otherwise be. I could enumerate many of my patients who have suffered from what might have been called very severe attacks of small-pox, and yet now they present not the slightest trace of having had the disease.

The following cases will give some idea of the results obtained while pursuing this plan of treating the malady.

Case 1.—Mrs. T., aged 60, a thin and delicate lady, took small-pox on November 21st, 1871, which was not only confluent, but in some parts of the body hemorrhagic. I never expected that my patient would recover, as, previously to this illness, she had been in a most critical state of health. Every portion of the body where the eruption made its appearance was painted over with a solution of one part of carbolic acid in twelve of glycerine. Immediately the great distress produced by the eruption was relieved, and was prevented from returning by the application being repeated night and morning. The rest of the treatment consisted in supplying plenty of fresh air and a simple yet nourishing diet. Chlorate of potash in solution was given as a drink, and the bowels were kept moving by a mild laxative given when required; and the patient passed through the whole course of the disease without an unfavourable symptom. There was no itching of the skin, and there was no secondary fever; indeed, there was no fever at all after the first application of the carbolic acid and glycerine. The patient slept well, and took her food with a relish. Within a year after her recovery, it was almost impossible to find any traces of the disease, so completely had the tendency to pitting been overcome.

Case 2.—Mrs. Y., aged 32, a strong and stoutly made lady, was confined of a healthy boy on December 7th, 1871, and on the day following was attacked by confluent small-pox. Under the circumstances, the greatest danger was, of course, to be apprehended, and I was, therefore, exceedingly anxious. The same treatment as in Case 1 was employed, and with the like satisfactory results, with the one exception that, at this date, very slight pitting can be perceived, if looked for. The baby was vaccinated before it was twenty-four hours old, and it did not take small-pox.

One more case will suffice to show that this treatment is deserving of a more extensive trial. On December 20th, 1872, I was asked to take charge of two ladies, mother and daughter, suffering from small-pox, their own medical man declining to attend. I found the mother prostrated by an attack of confluent small-pox, and her daughter suffering from the same disease, but of the discrete variety. The features in the elder patient were quite obliterated. The same treatment was adopted in both cases, with the effect of giving almost instant relief. As the disease held on in its course, the last named patient showed slight symptoms of prostration; and, although I was not apprehensive myself, I thought it better to have a consultation with the gentleman who then had charge of the Fever Hospital in this city. He took a very unfavourable view of the patient's condition, and gave it as his opinion that the case

would probably terminate fatally when the secondary fever set in; but, as this symptom never showed itself, the danger was avoided, and my patient made a rapid recovery. It is now impossible to detect any disfigurement from pitting.—*British Medical Journal, Nov.* 25, 1876, *p.* 677.

18.—ON VACCINATION.

By the Editor of the Lancet.

The arrangements for vaccination, and the care with which it is carried out, differ greatly in different countries, and what is true of the practice here may not be equally true of the practice elsewhere. This premised, we proceed to deal with the questions *seriatim*:—(1) There is no certain evidence existing in the enormous experience of vaccination in this country of any harmful result having followed from the use of vaccine lymph taken from *infants*, who subsequently to vaccination may have shown signs of syphilitic, or scrofulous, or other constitutional taint; there is no trustworthy evidence of syphilis at any time having been conveyed here by proper vaccination, and the probability is overwhelming that wherever syphilis may have shown itself after vaccination the special virus of that disease had been accidentally inoculated by vaccination or an inherited taint had been roused into active manifestation by the vaccination. The care usually exercised in this country in the selection of vaccinated children from whom to select virus renders it highly improbable that any would be taken from children tainted either with scrofula or syphilis; hence, so far as this country is concerned, there is no sufficient reason on the grounds stated to prefer lymph taken direct from the cow (animal lymph), to vaccine lymph which has been propagated through a series of human beings (humanised lymph). (2) It is difficult to conceive better and more persistent results from animal lymph than those which are obtained by experienced vaccinators here from humanised lymph. In fact, up to a very recent date, even in the most practised hands, animal lymph was much less trustworthy in its results than humanised lymph. This difficulty, it has recently been stated (we simply repeat the statement without affirming it), has been almost entirely overcome by the more skilled advocates of animal vaccination; but it must be long before a like skill will become common to medical practitioners in general, and until it became possible to secure generally as large a proportion of successful results as are now obtained from humanised lymph, it would be madness to attempt a large substitution of animal lymph in this country, where vaccination is compulsory, for humanised lymph. Moreover, re-vaccination is desirable after vaccination with ani-

mal as with humanised lymph — a fact which indicates that vaccination with animal lymph affords no "safer" protection than vaccination with humanised lymph. (3) In this country there is a difficulty which is now being attempted to be overcome by the exertions of some private individuals. Hitherto, here, animal vaccination has been, as a rule, resorted to, in order to renew stocks of humanised lymph which, from want of careful cultivation or other cause, have been held to have degenerated. But it is probable that, if these private efforts should succeed, animal vaccination may be more largely resorted to. The attention of the Government has again been called to the question during the present Parliament, and there is unquestionably a growing feeling in favour of the practice among both the profession and the public. But so long as the question stands as we have placed it in our answers to questions No. 1 and 2, it is unlikely that animal vaccination will become other than a supplementary matter to the practice followed in this country. (4) After successful vaccination in infancy, re-vaccination is needed once only. The second operation should, if practicable, be performed at the age of puberty, or, there being immediate danger of small-pox, at twelve years of age. Re-vaccination at an earlier age is futile. Repeated re-vaccinations are foolish. Re-vaccination, when successful, is needed once only. The operation should be renewed at intervals until it is successful, or until immunity from its effects can be safely assumed.—*Lancet*, *March* 24, 1877, *p.* 443.

19.—PREVENTION OF PITTING IN SMALL-POX.

By Dr. Robert J. Cooper, Ladbroke Road, Notting Hill.

I have used many applications to prevent pitting in cases of small-pox, but none from which I have derived such manifest good results as from common linseed-meal poultices assiduously applied to the face from the moment the eruption shows itself until the fever begins to decline. Among the many advantages that the poultice possesses over other applications, is the by no means unimportant one of softening and determining to the skin, and thus aiding the development of the pustules, and in this way relieving the system of the strain placed upon it. Anyone who has seen much of small-pox must have learned to look with anxiety for the appearance of well thrown up pustules on the face, knowing well the likelihood of a coincident relief to respiration and decline of temperature. This the poultices hasten, I had almost said effect; while, more than this, they exclude the light and completely prevent subsequent pitting.

My mode of proceeding has always been to have a saucepan with linseed-meal and water on the fire in the bedroom, and,

so soon as the poultices dry on the patient's face—and this, in really severe cases, they do very speedily—I direct the attendant to take some of the linseed-meal paste from the saucepan and apply it as a fresh poultice. In this way, the poultice is always ready.

It may be said that this application would be offensive, but let any one try it in a really serious case of commencing confluent small-pox, where the skin of the face burns with the deterioration of blood caused by the effort to throw out the eruption, and I am sure that, however disagreeable it may appear in description, in practice it gives almost heavenly relief. I do not question the advisability of bathing the face with a carbolic acid wash between each poultice; but, contrasted with the poultice application, I doubt if carbolic acid alone would be equally satisfactory.—*British Medical Journal*, *Dec.* 30, 1876, *p.* 856.

20.—TREATMENT OF INTERMITTENT FEVER BY SALICIN.

By Dr. W. Thompson, Consulting Surgeon to the Peterborough Infirmary, Surgeon to the Great Northern Railway, &c.

On making the acquaintance of Dr. Maclagan at Dundee last spring, and discussing the now recognised curative power of this remedy in acute rheumatism, I gave it as my opinion that we have in salicin a remedy equally energetic in staying or cutting short the paroxysm of ague. I determined that as soon as cases presented themselves I would give it a fair trial, acting on Dr. Maclagan's suggestions as to its administration in full doses; and I hope that the following cases will prove that the failure of the old-fashioned decoction of willow-bark, both in the hands of the inhabitants in the marshy districts, and of the medical men, was from the want of concentration, or the inability to prescribe a sufficiently large dose.

Case 1.—R. B., aged thirty-two, has been twice under my care, first in November, 1875, and secondly in March, 1876, for severe attacks of ague. He was a native of the Isle of Ely, Cambridgeshire; was treated with quinine, and was three weeks in hand on both occasions, and that he stated to be a shorter time than usual.

On the 2nd of October, I was sent for to see him at 10 a.m. I found him in bed shaking all over, his teeth chattering, and wearing that unhappy expression of countenance one invariably finds in these cases. Skin moist and clammy; temperature 103°; pulse 110, weak and frequent. He had a hot bottle to his feet, and had taken hot port wine without feeling better. I told him I was going to send him a new remedy, and he was to be very particular in following the directions, and noting his

feelings after each dose. Thirty grains of salicin to be taken every two hours. I saw him again at 7 p.m., six hours after the first dose. Temperature 99°; pulse 80. He said he felt much better. Half an hour after the first powder the chilliness began to abate; four hours after most of the unpleasant feelings had vanished, and he thought he need not go on with the medicine. Ordered to continue the salicin every four hours.

Oct. 3rd.—10 a.m.: Found him up by the fireside, looking weak and tired, but saying he feft comparatively well. I told him to be careful, as next day he might have a relapse.

4th.—10 a.m.: Found him as on my last visit. He, however, told me that at six in the morning he had "the same old feels" come on, and had it not been for the powders (two of which he had taken), he was sure he would have had a relapse. A powder to be taken night and morning, and to come and see me on the 6th, which he did, looking much better. To stop the medicine after to-day. Seen again on the 8th, after which he went from home, and when he came back, said he had not had such a short autumnal touch for five years, and asked me for some of the medicine to keep by him.

Case 2.—Jane G., aged forty, seen Oct. 10th in bed. Skin wet with perspiration, very shaky, can with difficulty sit up, has been ill every other day for a week. Thirty grains to be taken every two hours until I saw her again, which I did on the morning of the 12th, when she gave me the following account:—"After the second powder the perspiration stopped, and after the fourth the chilliness had gone, and the stiffness in the limbs was much relieved." Next day, as was to be expected, she was comparatively well. Next morning she was surprised to find herself equally so. To stop the medicine in the evening, and only take it again if she has any premonitory symptoms. Four days after she was quite well.

Case 3 I consider very satisfactory. W. A., aged seventy-four, has been subject to ague at times for years; has now been six months under treatment, quinine having been administered in full doses, until he vows he will take no more, as he believes he feels worse after it. Nitro-muriatic acid agreed better. The paroxysms of ague occurred every three or four days, and generally three times a day. As the next would be his bad day, I ordered him to begin the salicin at once (thirty grains every two hours, if awake). Saw him at 4 p.m. next day. Has had only one fit, and that the powder seemed to stop short; feels and looks much better. To take thirty grains every four hours until after his next bad day; if not well then to continue the medicine until seen again. Visited on the 9th November, 1876. Said he had a slight feeling of chill on the fourth day after beginning the salicin, but yesterday (the 8th)

felt as well as he does to-day, and that is better than he has done for six months; in fact, he says his limbs are not so stiff as usual, and he walks much better.

As the last two cases were in the Fen, five miles distant, I could not see them sufficiently often to note with the thermometer the antipyretic effect, or to record the cases in regular hospital form. I think, however, my first case shows salicin to have an almost magical effect in restoring the equilibrium of the circulation, making normal the temperature and checking this troublesome disease; while my last case shows it to be superior to quinine even in cases of genuine ague of old standing in aged patients.—*Lancet, April* 21, 1877, *p.* 567.

DISEASES OF THE NERVOUS SYSTEM.

21.—ON THE CURATIVE EFFECTS OF MILD AND CONTINUED COUNTER-IRRITATION OF THE BACK IN CASES OF GENERAL NERVOUS DEBILITY AND IN CERTAIN CASES OF SPINAL IRRITATION.

By Dr. Arthur Gamgee, F.R.S., Brackenbury Professor of Physiology in Owens College, Manchester.

I have long been convinced that physicians are very imperfectly acquainted with, and are far too sparing in their use of counter-irritation in a class of cases in which I have found it to be of immense service.

The preparation which I have almost exclusively employed to produce counter-irritation of the skin of the back in the cases now under discussion has been the Linimentum Sinapis Co. of the British Pharmacopœia. This preparation owes its activity both to the essential oil of mustard and to ethereal extract of mezereon. In consequence of the expensive character of the former of the two constituents, it is much adulterated, and the liniment made from it is not at all unfrequently almost inactive. When prepared with pure essential oil of mustard, the liniment should possess a very pungent odour, and should produce an almost painfully acute sensation in the nostrils when it is smelt. If properly prepared a few drops of linimentum sinapis sprinkled over a pad of cotton-wool ten or twelve inches long and four or five inches broad will suffice to produce in a few minutes pretty intense redness of the skin of the back, accompanied by more or less of the painful burning sensation characteristic of mustard. This method of applying the liniment is excellent, and is naturally suggested when it is employed in a sudden emergency in place of a mustard poultice. As a rule, however, where it is deemed necessary to keep up counter-irritation of the back for considerable periods of time, it is best to cause

the patient to wear a strip of spongio-piline four or five inches broad, and of the length desired. In the case of persons with tender skins the irritation and pain caused by even a very few drops of the liniment (which is diffused by sprinkling and rubbing one part of the spongio-piline against the other) is so considerable, that the application cannot at first be continued for many minutes. After a day or two the patient usually becomes able to bear the strip for several hours, and finds that the sensation of irritation is decidedly more pleasurable than painful. If, as frequently happens, the patient, having experienced benefit from previous applications, has sprinkled too large a quantity of the liniment upon the spongio-piline, the irritation produced may be so considerable as to compel an intermission of the treatment for a day or two. The irritated part then usually remains deeply congested and hot for several hours, only very rarely presenting any vesications.

The great advantages of the linimentum sinapis over any other similar preparation with which I am acquainted, lies in the fact that it produces a remarkably active irritation of the sensory nerves of the skin, which subsides to a great extent when the preparation is removed, but which can be renewed almost indefinitely without leading to any eczematous, pustular, or ulcerative condition.

2. *On the use of counter-irritation in cases of nervous debility.* Perhaps no class of cases demonstrates more convincingly the beneficial action of counter-irritation than cases of nervous debility.

Under the general and somewhat indefinite term "nervous debility," I mean to include cases in which, either as a result of long-continued bodily and mental exertion, or of undue excitement of certain cranial and spinal centres or of a disturbed nutritive balance of the organism generally, the individual becomes the seat of symptoms which indicate that the nerve centres are in an enfeebled condition, and specially that upon very slight exertion they manifest the symptoms of fatigue. Whilst the intellectual phenomena in these cases are very varying in character and degree, and very often are absent, rarely presenting themselves in a more obtrusive form than by a disinclination or inability for intellectual exertion, usually accompanied, it is true, by some sleeplessness, there are other symptoms which are much more constant; the inability to undertake even moderate muscular exertion, at any rate without distressing exhaustion supervening, the deep-seated pain in the dorsal and lumbar regions of the spine, and in males the diminution or abrogation of sexual passion, are amongst the more common symptoms. To these may be added neuralgic pains of the limbs, especially of the legs, and sometimes twitch-

ings of the muscles of the lower extremities. In the latter cases it would appear that the reflex excitability of the cord is increased, or that the inhibitory action of the encephalic over the spinal centres is diminished. Admirable examples of general nervous debility are presented to us by various classes of cases. We see them in women exhausted by rapidly-succeeding pregnancies or over lactation, we observe them as a result of sexual excesses, and they occur in an exquisite form in association with the intellectual or emotional phenomena of hysteria.

Without in the least wishing to disparage the use of those remedies which we are wont to employ in the treatment of these cases, and which, as in the case of iron, cod-liver oil, and phosphorus, probably act beneficially by supplying to an enfeebled and *materially* exhausted body the food which certain of its tissue elements require, I would insist upon the great value of counter irritation as a most important adjuvant.

In all the classes of cases referred to above I have tried the effect of more or less longer continued counter-irritation by mustard, and in all with an effect much more obvious than followed other methods of treatment, not omitting the judicious use of the constant current.

The general result of the use of this method of counter-irritation may be very shortly described. On the first or second day of the treatment, the patient, if a delicate hysterical girl, may complain that the pain caused by the mustard is almost unbearable, and she may declare that the application cannot be continued. By diminishing the amount of mustard-oil used, however, all such urgent objections on the part of the patient are removed. As soon as the application has been so controlled as to bring on merely an active glow and not unpleasant tingling of the skin, the patient declares that the increase in her strength is marvellous; the pains in the back and limbs undergo a diminution, or, as long as the mustard counter-irritation is kept up, are completely in abeyance, the irritability of temper diminishes, and simultaneously the general health undergoes marked improvement.

The increased feeling of vigour produced by the treatment is not illusory; as a rule I have found that the improvement thus commenced has kept up, so that a hysterical girl who had been for some weeks confined during the day to a couch to which she could with difficulty make her way from her bedroom, has in a few days cheerfully taken walks of considerable length.

In writing this description I have present in my memory the cases of several enfeebled or hysterical girls, one of a medical man, who, after a very severe attack of typhus, which seized him when his strength had been severely taxed by an unusual

amount of mental work, presented many of the features of cases of spinal irritation, and two or three cases of nervous debility resulting from sexual excesses.

3. Admitting the accuracy of my statements—and here I would insist that they apply only to counter-irritation carried on according to the method which I have described—*can any rational explanation of the satisfactory results be given?* We are in the first place met by the great difficulty that we do not know the exact nature of the physical changes in the nerve-centres, or the exact nature of the altered nutritive processes in those centres, which coincide with the symptoms of general nervous debility and so-called spinal irritation, and in the second place by the difficulty of asserting with precision what will be the effect upon the system of a certain measure of irritation of sensory nerves. It may, however, be well to examine these questions by the light of our present scientific knowledge.

All acts of the body are associated with material change, and in any given kind of act in any given individual (assuming his condition to remain sensibly constant) the material change bears a certain proportion to the intensity of the activity. Some individuals are wasteful machines, the amount of their available activities being small in comparison with their material receipts; and similarly the individual organs of different individuals, according to their more or less perfect adjustment, out of a certain store of energy-yielding material produce results which differ in quality and quantity.

There is perhaps no exception to the rule that all acts of the body are separated from one another by periods of repose, the period of repose being perhaps always the period of increased nutritive activity. Thus, if we take the case of an organ which at first strikes us as continually in action—the heart—we find that it is at rest during a longer period than it is in action, and the period of mechanical inactivity is the period of high nutritive activity.

Continuous activity of any organ leads to *fatigue*, and fatigue appears to be due partly to an exhaustion of the store of energy-yielding material which every organ possesses within itself, but in great measure to the definite action of chemical substances, which are the results of the operation of activity upon certain structural elements of the fatigued part.

Those organs of our body of which the activities are uncontrolled by our wills cannot be easily fatigued, the conditions of their activity having been so regulated that of necessity a period of rest follows a period of activity. Thus we cannot directly induce fatigue of our hearts, because the very nature of the mechanism of the organ compels an alteration of rest and activity, and provides for a certain measure of constancy in the

total amount of work done, even when its distribution in time is altered.

Increased blood-supply, it has been said, corresponds with increased activity of the organs, and diminished blood-supply with rest, and the period of rest is the period of recuperation or restoration. At first there appears to be an inconsistency in the assertion, which disappears, however, when we consider it further. All operations of the body which are accompanied with the conversion of potential into kinetic energy are accompanied by oxidation; oxygen is required in increased amount, then, during periods of activity; and it is very probable that the increased blood-supply of the active organ has for its chief objects to supply an abundance of oxygen, to remove the large quantities of carbonic acid, and the smaller quantities of waste substances, which are injurious and fatigue-inducing.

I would not, of course, commit myself to the absurd statement, that the restoration of energy-yielding matters in organs does not occur *pari passu* with its consumption, for it is probable that always the two processes of integration and disintegration proceed side by side (in activity the latter, and in rest the former preponderating); but what I would maintain is that although the amount of blood, and therefore of nutritive matters, supplied to the active organ is absolutely larger than that supplied to the inactive organ, the relative amount of matter available for restoration is greater in the latter than in the former case. An abundant blood-supply, probably, is followed, *as a necessity*, by large tissue changes, and therefore by loss of energy, and the period of restoration is the period in which income exceeds expenditure and stock is accumalated.

It will be remarked by the reader who is acquainted with physiology, that my reasoning is based very much upon that which is known to occur in muscle; that it is applicable, however, to the organs of the nervous system can be easily shown.

The activity of the brain is like the activity of a gland accompanied by an increased blood-supply; at any rate we know that functional inactivity, as it is manifested in sleep, is accompanied by a greatly diminished blood-supply. That which is unquestionably true for the brain is almost certainly true of the spinal cord. Now up to a certain point the greater the activity of the brain during a certain limited period, the greater is the subsequent tendency to sleep; in other words, the greater has been the call upon the energy-yielding store of the organ, the greater tendency there is for it to pass into that condition in which expenditure sinks to a minimum.

If, however, the activity of the brain is carried beyond a certain point, the fatigue of the organ leads to a dream-disturbed sleep, or to a more or less prolonged sleeplessness,

followed by sleep, and in either case, when the sleeper awakes, he will probably be still suffering from some symptoms of nervous fatigue; there has not been complete restoration.

Now what is the proximate cause of the dreaming and the sleeplessness? Almost certainly, it is due to a continued hyperæmia of the brain-centres. As a result of over-exertion, the blood-vessels of these centres lose their power of returning to their original size, and as with the continuance of a large blood-supply we have a continuance of brain activity manifested by dreaming or actual sleeplessness, restoration is not possible, on this view, as long as the blood-supply is too abundant.

But we know how serious are the results to the individual if the causes which lead to sleeplessness from overwork remain long in operation; he passes then into a condition of almost chronic brain-fatigue, and he becomes subject to neuralgias, to insanity, and to disorders of the general nutrition.

In the preceding paragraphs I have intended to show that the way in which excessive brain-work injures is, at first, at any rate, by leading to a certain amount of vaso-motor paralysis of the vessels of the encephalic centres. I would now extend that reasoning to the spinal cord, and argue that in all probability the excessive and prolonged activity of its centres lead to the same results as are observed in the case of the brain, viz., to continuous hyperæmia, and the latter is inimical to restoration. Thus I would argue does excessive and abnormal activity of the centres connected with the organs of generation lead most probably to the symptoms of nervous debility, as these centres are spinal, but in relation to psychical centres of activity, the results of their excessive activity may be either spinal or cerebral, according to the nature of the individual.

That hyperæmia of the cord accompanies the general debility which is observed in the libertine, or in the hysterical girl, appears to me very probable indeed. The vague neuralgic pains in the limbs, the deeply-seated back-ache, the spasmodic twitchings which sometimes occur, are probably indirectly produced by hyperæmia of the nerve-centres, and are symptoms which are more probably central than peripheric.

I come now to discuss the mode of operation of extensive but mild counter-irritation of the back in cases of general nervous debility.

As I said in the earlier part of this paper, the observations of physicians confirm the experiments of physiologists, in so far as the effects of irritation of sensory nerves are concerned. A moderate stimulation of the sensory nerves of the skin appears as a most general result to lead to increased vascular tension, due to an increased activity of the vaso-motor centre, the con-

striction of small arteries which occurs in the body generally being often accompanied by a local vascular dilatation of the irritated part. Now it is true that in experiments on animals it is often found that after a preliminary stage of increased vascular tension, due to the irritation of the central end of a sensory nerve, there is a subsequent reactive dilatation which we may explain as either due to a vaso-motor paralysis or to an increased activity of the vaso-inhibitory nerves; but it is quite conceivable that when we are able, by the aid of the sensitiveness of man, to regulate the cutaneous irritation so as to produce only very moderate irritation of the vaso-motor centre, there may be produced a tonic increase of vascular tension, lasting as long as the excitation is kept up.

If it be true that the symptoms of nervous debility and spinal irritation are, at any rate in great part, due to a lowered vascular tone of the nerve-centres, we can easily conceive that benefit should result from such counter-irritation as I have recommended, for the vessels of the nerve-centres will gradually be restored to their normal tone, their abnormal excitability will be depressed, and they will pass into a condition in which restoration is possible.

I have said in the course of this paper that an effect of counter-irritation of the back is to cause the patient to feel increased general strength, and that this frequently leads to excessive applications of the rubefacient.

Now this feeling of well-being appears to be wonderfully like that which cold bathing produces, only not so evanescent, and still more like the feeling which a constant current applied to the skin of the back induces. Now in both these cases the vascular tonus is doubtless largely increased for a longer or shorter time.

The healthy, vigorous man, who is in condition, has a tolerably high arterial tension; his vaso-motor centre is in a state of powerful tonic excitation, and his vaso-motor nerves are not readily inhibited (he cannot be easily made to blush, for instance.)

The circumstances which increase our vascular tonus—as long as our organs are healthy—appear to add to our vigour. Why should it be so? If rest is the period of recuperation or restoration, if a diminished blood-supply be a condition of rest, then a high degree of activity of the vaso-motor centre should tend to health; by preventing waste of energy until each organ is needed (and then the vaso-motor nerves are either centrally or locally inhibited or antagonized), by keeping each organ at rest until its activity is needed, there will be a greater store of energy-yielding material at disposal, and the activity of the tissue elements will be all the more perfect in that they are

uninjured by the fatigue-producing substances which are the products of activity: in other words that man will, *cœteris paribus*, be the most efficient and economical machine whose organs are the seat of the smallest waste *as long as they are inactive:* naturally, however, the quality and quantity of the work will depend very much on the quality of the organ, on its power of storing-up, and on the perfection of the vascular mechanism, in so far that this must permit of as large a supply of blood being given at the required moment for the maximum conversion of energy.

There are two points upon which I think it right to add some observations, so as to prevent my being misunderstood.

1. The facts which are day by day being accumulated point to the existence of local vaso-motor mechanisms, which are subordinated to the general vaso-motor centre (if such a centre really exists apart from the former), so that it is not only conceivable, but certain, that an affection of, or a change in, a local vaso-motor mechanism might be induced without a necessary affection of the general vaso-motor mechanism. A general high tonus does not, for example, imply that its possessor should have a tendency to sleep, for sleep is doubtless due to a positive cerebral anæmia, to the production of which the activity of a centre, specially connected with the cerebral circulation probably contributes.

2. Whilst such counter-irritation as I have recommended appears to exert a tonic action on the vaso-motor centre, and on the body generally, I am quite aware that several methods of counter-irritation produce an opposite effect. The shock and great depression which, especially in women, sometimes follow the application of blisters, seem to show that severe counter-irritation very frequently tends to depress rather than to raise the tone of the vascular system.—*Practitioner, Feb.* 1877, *p.* 113.

22.—ON SCIATICA.

By Dr. Thomas Buzzard, Physician to the National Hospital for the Paralysed and Epileptic.

The term "sciatica" is very widely applied. It may designate, first, and perhaps most properly, paroxysmal pains in the course of the sciatic nerves, for which no immediate cause can be discerned. Secondly, pains of a like description, which depend upon some cause of irritation applied to the trunk of the nerve itself, as, at all events, one factor in their production. Thirdly, pains in or about the muscles, and having only an indirect relation to a particular nerve supply.

Amongst predisposing causes of sciatica which require to be remembered in reference to the question of therapeutics are:

arterial degeneration, anæmia, the rheumatic and the gouty diathesis, the toxic influence of malaria, chronic alcoholism, syphilis, lead, and the inherited neurotic disposition.

As immediately exciting causes, the most potent of all (apart from irritations produced by wounds and destruction of tissue caused by growths) are fatigue and the influence of cold. A medical friend attributes his attack to his constantly alighting from a high gig upon the same foot. The affection often follows an unusual strain in walking great distances. The influence of cold, and especially damp cold, in starting or prolonging an attack does not need to be dwelt upon. Concussion of the spine may induce well-marked sciatic neuralgia.

The following seem to be amongst the most usual causes of peripheral irritation:—malignant and other tumours of the pelvis, disease of the vertebræ or pelvic bones, the pressure of the gravid uterus, rheumatic or gouty inflammation of the sheath of the sciatic nerve, or of one or other of its branches, syphilitic periostitis, causing a swelling which presses upon the trunk or some branch of the sciatic nerve.

There is a form of sciatica of peculiar intractability which is not generally described, I think, in works upon the subject. It precedes or accompanies chronic osteo-arthritis (*arthritis deformans*) of the hip-joint, and the sciatic nerve may sometimes be felt to have hard, knotty swellings in it. It would seem to offer an example of *chronic* neuritis,—malignant tumours encroaching upon and involving the nervous plexus, giving rise, perhaps, to *acute* neuritis. But we do not know much about the relation of neuritis to neuralgia. It is curious that in sciatica from these last causes (permanent as are the lesions) there are yet, as in the disorder of purely central origin, marked fluctuations in the severity of the pain. I have often seen sciatica, dependent on pelvic cancer, imitate closely the more common disorder in the paroxysmal character of its pains and intermissions. This circumstance indicates the necessity of repeating from time to time a careful examination, of the region, when early search has failed to find evidence of tumour. The diagnosis is often difficult enough with this precaution, but without it there is constant liability to error. Lastly, sciatica, when it involves great powerlessness of the limb, is more often mistaken for some obscure form of spinal paralysis than some of my readers would imagine.

The last of the three very broad classes into which I have ventured to distribute cases of disorder to which the term sciatica is conventionally given, is one which it is very important, for therapeutical reasons, to separate from the other two. It includes the following conditions:—

a. Myalgia, from over-exertion of the flexor muscles at the

back of the thigh. The pain is situated especially at the points of attachment of the muscles, and is only felt when the muscles are in action.

b. Muscular rheumatism in the same muscles, or in those of the leg, from exposure to cold. Here again the pain is not spontaneous as in sciatic neuralgia, but is only provoked by movement.

c. An irritated, perhaps low inflammatory, state of the loose bursal tissue, which intervenes between the muscles and permits of one sliding easily upon another. These are now recognised as lymphatic spaces, and are doubtless liable, as are the glands, to inflammation from the introduction into them of morbid material taken up by the lymphatic vessels. I owe to Dr. Burdon-Sanderson the suggestion of this possible cause of pain in the region of muscles. A case which I have had lately under treatment of what would perhaps be called gonorrhœal rheumatism seems to me to afford incidentally strong corroboration of this view. The symptoms embraced acute pain, tenderness on pressure, and remarkable muscular powerlessness, at first in one forearm, then in the scapular muscles especially, but not exclusively, of one side, later in the region of the *latissimus dorsi* of the opposite side. The pain only occurred on motion. Its situation, however, and the powerlessness of the arms had given rise to a suspicion of spinal meningitis. The affection of each district subsided after a few days without leading to suppuration. The patient was suffering from gonorrhœa, the discharge of which would subside at times and then recur. The muscular attacks coincided with the recurrence of the discharge. I am inclined to think that the connection of sciatica with gonorrhœa, which has often been noted and variously explained, may be best interpreted in this way. Probably it is not a sciatic neuraglia at all, but an inflammatory state of the lymphatic spaces in the sciatic region induced as it was in my patient. We may thus also without difficulty explain the nature of a large number of those cases of lumbago which are clearly not of neuralgic character, the origin of which has been hitherto puzzling, and which, as is well known, are cured by purgatives. Such cases are apt to occur in persons who have been indulging in good living, whose excretory organs are working imperfectly, so that irritating material, uric acid or some equivalent, is retained in the system. This being carried into the lymphatic spaces between the muscles of the loins, sets up a low inflammatory condition. It often happens that the lumbago of this kind is followed by sciatica which is curable by the same measures. Perhaps, too, in this manner, rather than in the removal of scybalæ, may be explained the not unfrequent cure of so-called sciatica by purging.

We have glided insensibly, but not inconveniently, into the question of treatment. Referring to the preceding lines, it would seem that in a supposed case of sciatica we must discover first whether the case is one of neuralgia, next, if it be so, whether the neuralgia be immediately or only secondarily of central origin. If the former, we must attack with suitable treatment conditions of system which show defects; if the latter, we must seek by careful examination for the source of peripheral irritation. Where this is incurable, as *e.g.*, in the case of malignant growths, we must employ palliatives for the relief of suffering; where, as for instance in the case of syphilitic new formations, it is removable, we shall of course use the appropriate remedies, iodide of potassium or mercury. The myalgic cases, on the other hand, will need remedies adapted to their particular character,—purgatives, as I have already said, and especially mercurial purgatives having exceeding value in the class depending upon irritation of lymphatic spaces by retained excretory matter. Muscular rheumatism may often be cured by induction currents applied to the skin by the wire brush, or by sponging the part with a negative rheophore of a constant-current battery, whilst the positive rheophore is kept applied to the skin. I do not propose to go further into details of systemic and specific treatment, but a few words respecting three methods especially directed to the relief of pain may perhaps be excused. In almost every case of sciatic neuralgia treatment of this kind will be a necessary complement of that directed to the probable cause of the disorder.

Of the three methods, none of which present novelty, hypodermic injections of morphia doubtless hold the first place. For my own part, I prefer employing very moderate doses, beginning with gr. $\frac{3}{30}$, and rarely finding it necessary, except in malignant disease, to exceed gr. $\frac{8}{30}$, a dose which is only arrived at by degrees. The solution I employ contains gr. i in ℥ xxx., which is much more convenient than the pharmacopœial strength for giving small doses, and appears to me to be more active in proportion than the more concentrated form. The side of the buttock is a convenient place for the injection, which should be repeated, I think, as the effect begins to wear off. In very severe cases twice in the twenty-four hours is usually sufficient; when the disorder is less acute once a day is often enough.

Flying blisters in my experience are frequently of extreme service. They need not be larger than a five-shilling piece, and one should succeed another as it heals, not on the same, but on a closely adjoining part of the skin. In this way the posterior part of the thigh may, if necessary, be gradually traversed

from above downwards. They may even be usefully employed as palliatives in cases not admitting of cure. A poor woman with agonising sciatic neuralgia from pelvic cancer found longer lasting relief from this treatment than from any other which I adopted.

The electrical treatment of sciatica is deserving of much more systematic and carefully-directed application than it has hitherto received in this country, except at the hands of a comparatively small number of medical men who have especially studied the subject. As I have already said, *induction currents* may be advantageously applied in those cases in which the affection depends upon muscular rheumatism. In cases of neuralgia, however, and especially in that class which is attended by local inflammation in the sheath of the nerve, I have found this form of electricity not only serve no useful purpose, but sometimes even apparently increase the symptoms. It seems necessary to say this distinctly, because, even at the present day, the notion of electrical treatment is limited in the minds of some to the application of magneto-induced currents from a rotary machine. I must admit that great good has been reported as having followed such applications, but I have seen nothing but disadvantage from them in cases which were of strictly neuralgic character.

The *constant voltaic current*, on the other hand, may be employed in this class, not only with rsesults which are often strikingly palliative of suffering, but which are also sometimes curative, it may be rapidly, but more frequently after a considerable number of applications.

In what description of case is this mode of treatment indicated? The question is one which, I regret to say, the materials hitherto collected do not enable us to answer with confidence. And the difficulty is largely dependent upon that confusion in the application of the term sciatica which this communication seeks to diminish. Cases dependent upon muscular rheumatism have been cured by electricity; cases of malignant disease have received no benefit; syphilitic affections of the nerve have been absolutely unaffected by it,—a variety, therefore, it is probable, of totally different ailments have been treated under one name. My own personal experience of electrical treatment has not been large, for the remedy is one which it has often been useless to prescribe, as the prescription stood no chance of being carried out, owing to the absence of materials. But I have now used it, in hospital and private practice, sufficiently often to entertain a strong opinion that the application of the constant current ought not to be omitted in any instance of sciatic neuralgia which does not readily respond to any systemic treatment which may be indicated by the special circumstances of

the case, coupled with hypodermic injections of morphia. I believe the application to be perhaps most hopeful in cases of sciatic neuralgia which are not dependent upon obvious sources of peripheral irritation; but this should not exclude its employment in the latter class in which (except perhaps when there is malignant disease) it may often prove of great service.

Details of application.—Any good constant-current battery may be employed—Dr. Tibbits' arrangement of the Leclanché, and Dr. Stöhrer's modification of the Bunsen, are probably the best for medical purposes,—and it will be necessary to use a current which proceeds from a tolerably large number of cells, say from about twenty to forty. The full strength of the current to be employed should be arrived at, however, step by step, to avoid the shock of its sudden application. Somewhat large sponge rheophores should be attached to the conducting wires, and thoroughly saturated with warm salt and water.

The object of the application, it must be remembered, is to expose the sciatic nerve to the continuous action of a current which is, as far as possible, of uniform strength, for a period of about ten or fifteen minutes. To avoid shocks during the application, neither rheophore should be lifted from the skin, and when the "sitting" is finished, the strength of the current should be gradually reduced to zero ere the circuit is broken by the removal of a rheophore. Any interruption of the current will cause a shock, and this it is very desirable to avoid in neuralgia. The strength of the current, when the full number of cells is gradually arrived at, should be as much as the patient can bear without much inconvenience,—it should be very distinctly felt, however.

When a constant current is sent into a nerve it developes what is called an "electro-tonic current" beyond the rheophores (not between them), and the irritability of the nerve is modified in a peculiar manner. That portion which is in the neighbourhood of the positive pole experiences a marked loss of irritability, whilst that which adjoins the negative pole has its irritability increased. As we wish to diminish the irritability of the sciatic nerve at its origin, the proceeding described would appear to be most likely to effect the object. Practically, however, the relative position of the poles, and the direction of the current therefore, is not found to make the difference which might have been expected, and it is certain that pain may be allayed in the same person by a current which does not always take the same direction. As regards the duration and frequency of the applications, it is generally found that from ten to fifteen minutes is sufficient. The sitting should be repeated daily, or even more than once daily. In some cases great relief immediately follows; in others it never occurs. Cure may take

place very soon, or prolonged treatment may be required; and, according to my experience, it is impossible to form any idea beforehand, in a given case, whether, or to what extent, it will prove amenable to this mode of treatment

If the constant current be employed with the object of testing its efficacy, all other treatment should of course be omitted, but as our business in life is probably quite as much to relieve suffering without loss of time as to advance science, we shall do well as a rule not to omit the hypodermic use of morphia, the employment of rest, and of any systemic treatment which may be indicated, whilst the very potent influence of the current is added to these measures of relief.—*Practitioner, Feb.* 1877, *p.* 94.

23.—ON OIL OF TURPENTINE IN SCIATICA.

By Dr. W. Allan Jamieson.

Sciatica, in our cold and damp climate, is common enough; it varies with the age, temperament, and diathesis of the person affected, while its treatment as yet is almost entirely empirical. Adopting Dr. Anstie's classification, three main groups may be recognised. The first of these is that occasionally seen in young persons, most commonly females, though by no means unknown in men. While the constitution of these patients partakes more or less of the hysteric character, the exciting cause may be often found to exist in some peripheral irritation of branches of the sacral plexus, more especially of those distributed to some part of the sexual apparatus. It is not a common form of sciatica, and I cannot at present recall a very typical instance in my own practice. Perhaps the following case is the best illustration which has come under my notice:—

M. S., æt. 24. Domestic servant. Of a reddish-fair complexion. Has been for more than a year in the service of a family in Edinburgh, who took her with them to an elevated situation a few miles from Dunbar in the beginning of August. For three months before I saw her, on the 19th September, 1876, she had experienced pain along the right sciatic nerve. A medical man who attended her soon after it began, gave her temporary relief by subcutaneous injections of morphia. For a time the pain was endurable, and she went on with her duties, but it gradually became worse, preventing sleep, and incapacitating her from work. She was sent in to a sister in town, at whose house I visited her on the date named above. She then complained of severe pain in sciatic notch at knee and outer ankle, but, contrary to what is usually observed, she seemed unable to keep the limb still in bed, and the pain on

pressure behind the trochanter was elicited by slight contact. Her evening temperature was 98·8°, and her state otherwise normal. I injected $\frac{1}{4}$ grain of acetate of morphia deep into the gluteal muscles, and ordered her—

℞. Olei terebinthinæ, ʒ ij.; olei ricini, ʒ iv.; mucilaginis, ʒ iv.; aquam ad, ℥ ii.

The draught to be taken early next morning.

When seen on the afternoon of the 20th September, the morphia had given little relief, while, though the medicine prescribed had acted freely on the bowels, the pain in the sciatic was as great as ever, and the restless movement of the limb even more so. As her sister was from circumstances unable to wait upon her. she was admitted the same day into the Royal Infirmary, under Dr. Haldane's care, where her case proved a tedious one. There was here no history of rheumatism, nor of any such exciting cause as cold or damp, while the age of the patient and character of the pain raised at least a suspicion of an hysteric element, so that the failure of oil of turpentine to afford relief scarcely astonished me.

The features of the second form are well marked. It is especially that connected with the period of tissue degeneration. It is comparatively rare in my experience before fifty, though cases much resembling it are occasionally met with in persons between twenty and thirty. Ten examples, of which I have notes, since 1870, have been exactly divided between the sexes. General experience of all cases of sciatica prove it rather more more frequent among men. Pain often commences first in the lumbar and sacral regions, and then after a variable time creeps downwards into the parts more immediately supplied by the great sciatic nerve. Points tender to touch also manifest themselves successively from above downwards. Thus, those which first appear correspond with the terminations in the skin of the posterior branches of the sacral nerves; then the coccygeal, and perhaps the vesical branches, are next affected, and nearly simultaneously with these a fibular point over or near the head of the fibula; and, lastly, an external malleolar. This last is very persistent, continuing even when those higher up in the chain of distribution have almost or altogether faded away,—so much so that I have frequently seen sinapisms and even blisters applied over the external ankle in the vain hope of thus mitigating the pain there. Sometimes a painful point is developed in the centre of the calf, Cutaneous anæsthesia is also a symptom, but is often masked in the early part of the disease by the tenderness of certain portions of the skin becoming a prominent feature during its decline. The motor branches of the nerve are often, indeed, usually affected, locomotion being

impaired quite independently of the pain resulting from use of the limb, and consequent pressure on the nerve and its branches by muscular contraction.

The predisposing cause is in many cases long-continued pressure on the sciatic nerve—in females who sit much and take little exercise—in males whose occupation is sedentary, though not necessarily one within doors. I am inclined to think, too, that position in sitting may determine which nerve is affected. Nothing is more common than for an individual in a family to have his or her own seat, by one side or other of the fire it may be. In this way, one nerve is likely to suffer more from pressure than the other; and so, from local anæmia thus induced, or depression of functional activity, to be easily influenced by such an exciting cause as damp cold. Exposure to to this latter is usually the immediate precursor of an attack. In one of my cases the seizure was brought on by a long drive in an open gig on a raw cold evening, across a bleak Northumbrian moor, followed by a night spent in a draughty room with insufficient bedclothes. In another, a miller caught an attack by getting wet while assisting to drive a horse and cart out of the Whitadder, and subsequent carelessness in changing his clothes.

While no temperament or diathesis can be said to be free from liability to an attack, the class of persons most subject to this form of sciatica, and who derive most benefit from oil of turpentine, are those who present the following appearances :—

Usually middle-aged; they look older than they ought, and have hair, as Anstie so well phrases it, *prematurely and permanently gray*. The complexion may be pale or fresh, but it nearly always exhibits a peculiar *ashy gray* or *stone-coloured* aspect—a point to which I attach much importance. The proof of tissue degeneration may be more conclusively shown by rigid radial arteries, possibly by arcus senilis.

The following case, which was first seen by me on the 26th August, 1870, illustrates many of the particulars stated remarkably well.—

Mrs. W., æt. 56, a lady in comfortable circumstances, who had a family of six children, and up till then had not had any severe domestic trial or worry. She had, however, occasionally of late, attacks of the neurotic form of angina, and one of her daughters subsequently had neuralgia of a severe type in various groups of nerves. She was tall and stoutly made, rather pale, her complexion having the ashen hue which I have already mentioned. Her hair became gray early in life, and continued so. She never had gout or rheumatism. I afterwards observed that, when at home, Mrs. W. always occupied a large armchair, so that her right side was next the fire. On the 11th

August, 1870, had a smart seizure of choleraic diarrhœa, which lasted all night and left her weak. She was then living on the banks of the Tweed in a rather damp and relaxing situation. On the 22nd she began to have pain in the back, and along the course of the right sciatic nerve; three days after she was quite lame, so she returned to her home in Berwick, and I saw her on the 26th.

I found her with a clean tongue, but marked with many deep cross fissures; appetite fair; bowels, now perfectly regular. Pulse 60, equal, of good strength. There was severe pain along the course of the right sciatic nerve, paroxysmal in character, and when the attacks came on, there was also pain in the urethral branches; urine normal as regarded quantity and quality. A blister was applied behind the great trochanter, and rest in bed enjoined.

29th. Pain very intense, felt chiefly in calf and outer ankle. One twenty-fifth of a grain of sulphate of atropia was injected subcutaneously, which produced distinct physiological effect, and afforded temporary relief.

30th. Repeated atropia.

31st. Pain less; 15 grains of carbonate of iron twice a day.

3rd September. Pain still continues; 5 grains of iodide of potassium, with 8 minims of tincture of colchicum, were prescribed, to be taken three times a day; and as the nights were sleepless, half a drachm of chloral at bedtime. On the 6th there was some diarrhœa and tormina; the colchicum and chloral were omitted, but the iodide was continued till the 11th, when, as it appeared to be doing no good, it was given up, and the carbonate of iron resumed.

17th. Passed a very bad night; did not sleep, and suffered much pain in coccygeal, vesical, and malleolar branches. Injected $\frac{1}{4}$ grain of acetate of morphia, which relieved, but gave rise to much nausea. 40 grains of chloral at bedtime.

18th. Slept well, but pain undiminished. The chloral draught was repeated, and the following mixture directed to be taken early in the morning:—

℞. Ol. terebinth., ʒ ij.; ol. ricini, ʒ iv.; tinct. cardam., co., ʒ i.; mucilag. et aq. ad ℥ ii.

19th. The medicine acted freely; the pain is much abated—so much so, that Mrs. W. can now lie on her side The mixture was repeated on the 21st, and again on the 25th. Next day the pain was so nearly gone that she could sit comfortably. A slight degree of numbness persisted for some time; but recovery was uninterrupted and complete, no fresh attack having since occurred.

In this instance the effect of the oil of turpentine was so real,

as compared with that of all the other means which had been tried, that my patient, when I visited her after the first dose had acted, accosted me with the exclamation, "Why did you not give me that medicine at first?" Its nauseousness had in fact deterred me, but I learned a lesson not to hesitate in ordering it on that account. Since then, I have employed it as the first remedy in eleven cases, all of them presenting in greater or less degree the characters of the class detailed above. In ten of these cases, relief from pain was immediate, not in any entire or complete after a single dose, but such an amelioration as to render lying on the side affected, or even sitting up, quite possible. A second or third, in several even a fourth dose, was required to reduce what was previously intense suffering to a state of slight numbness. In one man it failed to cure, though his case seemed well suited for its employment.

T. S., æt. 61, was seen by me last March, after he had suffered from sciatica in the right leg for some weeks. He was a man of by no means temperate habits, and was employed in a large wood-yard. He presented both the gray hair and ashy expression. Turpentine gave him temporary relief, but the pains recurred. Blisters, acupuncture, iodide of potash, iron, and strychnia were tried in succession, without effect, but the pain finally yielded to subcutaneous injections of morphia, at first daily, then every second day. I think in his case there was a rheumatic taint; certainly it was one of the most obstinate I have met with.

Turpentine belongs to the narcotic stimulant class of remedies, and I agree with Dr. Chambers in thinking it acts locally, just as its action is also probably local on the unstriped muscular fibres of the intestines when, administered in the later weeks of enteric fever, it so rapidly and effectually reduces the tympanitis so distressing and dangerous. Turpentine, then, to do good in the sciatica of the period of tissue-degeneration, must be given in doses of at least two drachms, combined with half an ounce of castor-oil, suspended in mucilage, and rendered more palatable by the addition of an ounce or so of cinnamon-water. The draught is best taken early in the morning. The dose generally acts powerfully two or three times, but its action is not followed by exhaustion at all commensurate with its effect on the bowels. Sometimes there is considerable difficulty in keeping the mixture down; usually, however, this is less urgent when cinnamon-water is employed as the vehicle. In the treatment of all forms of sciatica, absolute rest in bed must be maintained, not only while the pain continues, but for some time after it is gone. This is oftentimes difficult, as patients rebel against what they consider an unnecessary restriction. The following, among others, is an

instance where I had reason to regret the neglect of this precaution.

J. B., aged 25, a masculine-looking female, an outworker on a farm in Berwickshire, began to suffer from sciatica in February, 1875. I saw her on the 20th March, when the pain was so severe as to confine her per force to bed. I ordered her the turpentine draught, and on 24th March received a letter stating that this had at once relieved her, and she had gone to work. She sent for me on the 28th, and was found worse than ever. The turpentine was repeated on the 29th, and on the 1st and 2nd April, and this time, warned by her previous experience, she rested longer, and got quite rid of her troublesome enemy.

Commonly the dose does not need to be repeated oftener than every third or fourth morning. I have never seen strangury produced by its use in sciatica, nor indeed any unpleasant consequences.

Lastly, there is a third group of sciaticas, in which the sheath of the nerve probably suffers from the action of a specific blood poison, be that gouty, rheumatic, or syphilitic. Of the first and last forms I have seen no examples. The rheumatic has been fully treated of by Dr. Fuller, who, however, names so many remedies as to lead to a doubt which is the right one. It is the form seen in persons long subject to rheumatic pains in various parts of the body. A collier, who supplemented his earnings more legitimately made, by lying behind dykes and under hedges, and having a shot at any chance hare or rabbit which might come in his way, came under my care a few years ago with a severe attack of sciatica. I have no notes of his case, but I remember that after every remedy I could think of —turpentine included—was tried ineffectually, he went into the workhouse, and came out crippled with his prolonged illness, but with his pain relieved by almost parboiling him in a hot bath. This group includes some of the very worst cases, which tax their attendants' patience and resources to the utmost.—*Edinburgh Med. Journal, March* 1877, *p.* 788.

24.—CROTON-CHLORAL IN NEURALGIA.

By Dr. E. Markham Skerritt, B.A., Physician to the Bristol General Hospital.

[The substance known hitherto as croton-chloral is really butyl-chloral, as shown by recent chemical researches of Dr. Oscar Liebreich. Dr. Skerritt has pretty freely used butyl-chloral, and the following article is founded chiefly upon notes of its action taken from his hospital case book.]

A brief summary of the *physiological* action of croton-chloral may serve as a useful preface to the record of its effect in the

cases that I shall proceed to relate. Liebreich has shown that this drug is essentially an anæsthetic, and in large doses a hypnotic; that anæsthesia always begins at the *head*, and only after this region is fully under the influence of the croton-chloral and sleep has been induced does the anæsthetic effect gradually extend over the rest of the body. With large doses the respiration and pulse become slower, and a fatal result is due to arrest of the *respiratory* movements, not to paralysis of the heart; in this respect croton-chloral differs from chloral, as with the latter death occurs directly from cessation of the *heart's* action.

We should from the above be led to expect that the most marked effects of croton-chloral as a therapeutic agent would be seen in the case of painful affections of the *head*.

I have prescribed the drug for about 120 hospital patients, and have made use of it in cases which may be arranged under the following divisions:—

A. Pain or other abnormal sensations about the face, head, and neck, including—(*a*) Ordinary headache, affecting the anterior part of the head, or the occiput and upper part of the back of the neck. (*b*) Megrim, migraine, or so-called bilious sick-headache. (*c*) Facial neuralgia and toothache. (*d*) Headache associated with cerebral disease. (*e*) Sensations such as giddiness, without actual pain.

B. Pain in other regions, comprising—(*a*) Pain other than visceral, in the limbs and trunk. (*b*) The pain of dysmenorrhœa. (*c*) Pain connected with digestion.

C. Hysterical convulsions.

A. *Pain or other Abnormal Sensations about the Face, Head, and Neck.*—(*a*) *Ordinary headache*, varying from slight attacks to very severe, the worst of the latter being known as migraine, and popularly as sick-headache, bilious sick-headache, or bilious attack. The affection in all these cases I believe to be essentially neuralgic. The nerve usually at fault is the first or ophthalmic division of the fifth; this supplies the skin over the temporal, frontal, and anterior parietal regions; the area supplied by branches of this nerve is that most generally affected in headache. The auriculo-temporal branch of the third division of the fifth is distributed over an area extending from above the ear to the vertex. So far the whole area is supplied by the fifth nerve. Passing backwards, we come to the region of cervico-occipital neuralgia, occupying the posterior parietal and occipital regions and the back of the neck. The nerves concerned here are the posterior branches of the first four spinal pairs, and especially the great occipital.

The following is a brief description of an attack of neuralgic headache. The pain at the beginning is generally limited to a small area of varying position, and is usually unilateral; it

may remain localised, but more often spreads from that spot over a greater or less extent of surface, and may affect both sides, or be simply hemicranial. It is not necessarily limited to the region of any one nerve, but may at the height of the attack affect the whole scalp. The pain, as is usual with neuralgia, varies in character and intensity. After the pain has ceased, the scalp is often left very tender for a day or two, so that female patients will frequently say that the head is so sore that they cannot comb out the hair.

Migraine, sick headache, or bilious headache, is an important variety. This is not the place to enter into a discussion of the pathology of this affection, but I may say that my experience has led me to adopt the opinion held by the late Dr. Anstie, that it is simply a neuralgia. Anyhow, it cannot be too distinctly remembered that it is a pure neurosis, and is not due to digestive disturbance. It is not an uncommon thing to meet with patients, particularly anæmic women, who, suffering from what they know as bilious sick-headache, fly for relief to so-called antibilious pills and the like, which, far from curing the malady that is the outcome of nervous exhaustion, add the element of digestive derangement, and thereby aggravate the original complaint. In migraine, the act of vomiting is, in fact, only an expression of the intensity of the nervous exhaustion; and this act may be associated with severe neuralgias of other parts of the body than the head. Dr. Anstie records cases of intercostal neuralgia and of neuralgia of the testis, in which the attacks sometimes culminated in vomiting.

I will begin my examples of the use of croton-chloral with a well-marked instance of migraine. (Unless otherwise specified, the dose in each case was five grains three times a day.)

Case 1.—Female, married, age forty-five. Subject to attacks of "the bile." This, on analysis, resolved itself into severe headache, going on to nausea, and sometimes to vomiting. Between these attacks of "bile" she frequently had headaches of less intensity. Menstruation had ceased for eight months. In a week, when I saw her next, she said she had had no attack of the "bile" since, but had been troubled with the headache —that is, the neuralgia had been deprived of its intensity. In another week, she stated that the attacks of headache were very much less frequent and less severe; and in another week she was quite free from all pain.

Case 2.—Female, single, age seventeen. Very marked anæmia and chlorosis, with the ordinary symptoms. Had not menstruated. She complained of headache, beginning at the right temple and spreading over the head; not accompanied by vomiting. Quinine and iron for a fortnight produced some general improvement, but had no effect upon the headache.

Croton-chloral was then substituted, and the headache had entirely disappeared in three days, and did not return.

Case 3.—Female, married, age twenty-four. For six or seven months had been subject to vertigo and pain in the temples, the attacks of pain coming on for about half an hour at a time, and then leaving her giddy, with a peculiar taste in the mouth; this would occur several times in the day. She always felt ill, and was languid and sleepy. Menstruation regular; no children. In a week, she said the headache was less frequent, less severe, lasted a shorter time, and left no giddiness behind. In another fortnight the pain had entirely left.

Case 4.—Female, married, age twenty-four. Subject to headache, beginning at the right side of the head, and thence extending into the ear and up to the vertex; limited to the right side. The scalp was very tender after the attacks. Cured in four days.

Case 5.—Female, single, age seventeen; anæmic; subject to headache and vertigo. Under iron the general condition improved, but the headache remained untouched. Croton-chloral almost entirely removed the pain in two days.

Case 6.—Female, single, age fifteen; somewhat anæmic. For three months had suffered from headache and giddiness, the pain affecting the right temple and forehead, and leaving the skin tender. The headache was speedily cured.

Case 7.—Female, single, age sixteen. For three months had had severe pain in left temple, coming on very frequently in the day, and lasting for about ten minutes at a time, with a sensation of great heat of head. Cured after two or three doses of croton-chloral.

Case 8.—Female, single, age seventeen. For about a fortnight had suffered from pain on the left side of the head, leaving the skin tender; also pain between the shoulders. The headache was cured by four doses; the pain between the shoulders was not relieved.

Case 9.—A girl, eight years old, of tubercular aspect. This is, I believe, the youngest patient to whom I have had occasion to give croton-chloral. For about three months she had suffered from constant frontal headache; this sometimes came on suddenly with such severity as to make her cry out. Sleep was disturbed; slight cough. The scalp was so tender that she could not bear to have the hair combed out. The history of this case was such as to raise a suspicion of threatening intracranial mischief; there was, however, no other evidence to be gained by examination of the patient, and the tenderness of the scalp pointed to simple neuralgia. I prescribed three grains of croton-chloral three times a day. Next week the patient was very much relieved. I then substituted bromide of potassium,

when the pain became worse, and on a return to croton-chloral the headache again almost disappeared.

Case 10.—Female, married, aged sixty-seven. This is an example from the other extreme of life. For about five weeks the patient had suffered from severe and almost continuous headache, beginning at the occiput and extending forwards, leaving the scalp very tender. In two days the pain was very much relieved, and there was no soreness of scalp after it. On bromide of potassium the head became bad again, but a return to croton-chloral was followed by immediate and marked relief.

Case 11.—This case is of interest as showing the association of vomiting with neuralgia of traumatic origin, and the effect of the drug upon the complaint. Female, single, age twenty-two. For some years had been subject to headache, which was sometimes very severe; it always began over the right eye, in the scar of a cut received when twelve years old. For the last two years the pain had been very great; it was sometimes accompanied by severe vomiting. There was a crop of herpes over the right eye. The association of neuralgia with herpes, and of herpes with an abnormal condition of the adjacent nerve, is well known; hence, I think, we may conclude that here the neuralgia was due to the abnormal state of the nerve consequent upon the wound, and that the ultimate cause of the vomiting was this local lesion. The bearing of this case will be seen upon what I said before with respect to the pathology of migraine. After taking croton-chloral for a week, the patient came back, and said she had had scarcely any pain since she had taken the medicine. Here we have an instance of the effect of the drug upon a neuralgic affection referable to a definite local lesion.

Thus far I have quoted cases of female patients only, and I need not say that by far the majority of neuralgic patients are women. However, in my limited experience with regard to the action of croton-chloral in cases of headache in male patients, the result has been more uniformly satisfactory than in the case of women. The following are examples.

Case 12. — Male, married, age thirty. For six or seven months had suffered from continuous frontal headache, with vertigo on looking steadily at any object. Complained of drowsiness; no other alteration of sensation; movement normal; optic discs unaffected. Tonics produced no improvement, but the pain was very much relieved by croton-chloral. He still felt drowsy at work.

Case 13.—A boy, age twelve. Headache for a fortnight, beginning at the side and spreading over the head. No vomiting. Cured in three days; no relapse.

Case 14.—Male, married, age thirty-three. For *three years* had suffered from pain in the head, which he described as "not exactly headache," beginning at the occiput and running forwards, sometimes on one side of the head and sometimes on the other. His knees were weak; no other history or evidence of disease. In a week he was very much relieved, and soon the headache was entirely removed.

Case 15.—Male, married, age forty. For six months had been troubled with pain in the left temple, darting in character, leaving the scalp extremely tender; he could not lie on either side to sleep, as his "ears got numbed." No other evidence of disease. In a fortnight he was cured. In the fulness of his gratitude he told me that he had been treated at another institution for three months without any relief (and had therefore, doubtless, had all the ordinary remedies), that he had been unable to take his food or read at all; whereas now his appetite was good, he could read small print without any trouble, and had not felt so well for twelve months.

Case 16.—Male, single, age thirty-three. For twelve months had had severe, almost constant, frontal headache that left the scalp quite sore. He was very giddy and nervous. In a week the pain and giddiness were almost gone.

Case 17.—Male, single, age twenty-one. For about a month had suffered from constant occipital pain, which was getting worse. There was no soreness of scalp. Had constitutional syphilis three years ago. The optic discs were normal. From the history I thought there might be some intracranial disease, and prescribed iodide of potassium; the pain, however, became worse, so as to interfere with sleep. I then gave croton-chloral with marked benefit; the headache recurred only occasionally, and was but slight when it did return.

Case 18.—A boy, age ten. Generally in bad health, enlarged glands in the neck, ecthyma about the knee, and altogether very much out of health; complained of frontal headache. The phosphate of iron had no effect on the pain, but croton-chloral removed it entirely.

I now pass on to the third sub-division.

(c) *Facial Neuralgia and Toothache.*—In these affections the nerves at fault are the second and third divisions of the fifth and their ramifications.

Case 1.—Female, single, age twenty-four. For three weeks had suffered from severe pain down the left side of the face. Croton-chloral relieved the pain in a marked degree.

Case 2.—Female, married, age forty-eight. Subject to frequent attacks of headache affecting the whole scalp, and leaving the skin somewhat sore; also complained of pain down the face, with tenderness of gums and a feeling as if all her teeth

were loose. Menstruation ceased two years ago. Bromide of potassium combined with bicarbonate of soda and gentian, produced no improvement; but croton-chloral in a week very much relieved the patient—the headache was but slight and left no soreness, the pain down the face was gone, the gums were not tender, and her teeth all felt firm in her head; she could eat well.

Case 3.—Female, married, age thirty-four. For a month had suffered from headache over the occiput and extending down the back of the neck, sometimes over the temples; also from faceache connected with carious teeth. Croton-chloral was given with marked effect, but the patient said she felt very giddy; she was not sure, however, whether the giddiness might not have been present before, as the pain was so bad that it would have hidden it. Bromide of potassium relieved this vertigo.

Case 4.—Female, married, age thirty-five; much troubled with faceache connected with very bad teeth. In a week very much relieved, but had some headache and giddiness. An increase of the dose to five grains four times a day soon cured both the faceache and the headache, and the patient said her head had not been so well for months.

In my experience croton-chloral has not been so successful in cases where the pain is dependent upon the presence of carious teeth as in those where the neuralgia is not due to any such definite local lesion; that is to say, although toothache proper *may* be cured by this remedy, yet this result is not to be confidently expected.

(*d*) *Headache associated with Intracranial Disease.*—The few cases in which I have given croton-chloral have not been relieved.

(*e*) *Sensations such as Giddiness in the Head without Actual Pain.*—The following two cases are examples:—

Case 1. Female, married, age twenty-eight. Complained of heaviness in the head, not actual pain, followed by soreness at the vertex. Very much relieved in one week.

Case 2.—For the notes of this I am indebted to my friend Mr. Pearce Gould, of London, to whom I recommended a trial of this remedy. Female, single, age twenty-two. For six months had suffered from constant giddiness, lasting all day—not influenced by position, exercise, food, &c.—but going away directly on lying down at night. No pain; no other complaint. No marked anæmia; venous hum present; menstruation irregular; had been under treatment for six months without benefit. Ordered bromide of potassium; this produced no improvement; the addition of iron was followed by no alteration. Ordered croton-chloral, five grains three times a day, and iron

as before. For the next fortnight there was some improvement; on some days the giddiness was quite absent—then it became so slight as to be unnoticed when the patient was occupied, and she said she felt "quite a different creature." Croton-chloral was then omitted, and the giddiness returned almost as bad as ever. A further short course of the drug cured her.

In one case of *mumps* I gave croton-chloral without any effect upon the pain.

B. Pain in other Regions.—Having now left the head, I regret to say that my experience of the effects of croton-chloral in the relief of pain in other parts of the body has not been favourable, though it is right to state that it has been much more limited than in the case of neuralgias of the head and face.

(*a*) *Pain other than Visceral, in the Limbs and Trunk.*—In cases coming under this head in which I have tried this remedy, including instances of sciatica, intercostal neuralgia, pleurodynia and lumbago, and rheumatic affections, there has been no definite improvement from its use. I may quote the following case:—Female, married, aged sixty-eight, suffering from rheumatic pains about the right ankle. I prescribed five grains every four hours. After some days there was no alleviation of the pain; but the patient said she slept more soundly, and also stated that the medicine made her feel very queer in the head and gave her headache.

(*b*) *The Pain of Dysmenorrhœa.*—There have been several cases recorded of the removal of this by croton-chloral; but my experience has not been satisfactory. The following case illustrates at the same time the influence of this remedy on the head, and the absence of effect as far as regarded uterine pain:—Female, single, age eighteen, anæmic, very fat and flabby. For twelve months, and especially for the last six, had suffered from pain in the sacrum, with abdominal pain on micturition. Menstruation regular, accompanied by sacral pain of the same kind, but more severe. Was also subject to sick headache. Under croton-chloral the *head* was cured, while the *sacral* pain was unrelieved.

(*c*) *Pain connected with Digestion.*—Here also I have had no encouragement to prescribe croton-chloral. Example:—Male, single, age twenty-two. For two months, frontal headache, usually coming on after food; pain after food going through from the epigastrium to the back; memory and sight defective; patient was very nervous. The *head* was very much relieved by the medicine, while the *epigastric* pain became rather worse.

C. Hysterical Convulsions.—In the three cases in which I gave croton-chloral for this condition there was no effect. In one of them the fits were always preceded by headache; but, though the headache was relieved, the fits were not influenced.

The Induction of Sleep.—I have not used this drug as a hypnotic simply, as the dose required is very large; according to Liebreich, it varies from fifteen to sixty grains. Instances have, however, been recorded where very small doses have been followed by sleep; in one case two grains was said to have proved most effectual. I cannot but think either that there was some peculiar predisposition to the action of croton-chloral in such cases, or that they were simply illustrations of *post hoc, propter hoc.* I have given very much larger doses for days together without any hypnotic effect, and have myself taken twenty grains at a dose without any perceptible result. In about half a dozen cases, out of the whole number, have the patients complained of feeling drowsy after the medicine, and they were taking at least fifteen grains in twelve hours.

Dose and Mode of Administration.—Croton-chloral is but sparingly soluble in water, but to a sufficient degree to make a solution as strong, in my opinion, as any patient will be likely to take, as it is the reverse of palatable. Ten grains will dissolve in the ounce of water without much difficulty; glycerine makes it rather more soluble. Dr. Yeo gives four grains to the drachm as the strongest solution that can be made with water and glycerine. At the hospital Mr. Berry uses an alcoholic solution. The drug may also be administered in the form of pills.

There does not seem to be any risk from large doses of croton-chloral. Dr. Ringer has given five grains to a patient every hour for a fortnight, and Dr. Liebreich has prescribed a sleeping draught containing about a drachm and a half.

In hospital out-patient practice I generally order five grains three times a day, and have found that patients unrelieved by this dose are often very much benefited by the addition of another dose of five grains per diem. Elsewhere I have given the same dose every two, three, or four hours, according to the urgency of the case.

The only unpleasant effects I have observed have been the following. In two cases, vomiting; in one of these this was so constant after the dose that the medicine had to be discontinued; in the other it ceased when the drug was taken immediately after meals, and the desired effect was obtained. In several cases, drowsiness, but not so great as to necessitate the discontinuance of the remedy. In two or three cases, giddiness; in one, headache.

From the details that I have given it will be seen that in my experience the results of the therapeutic use of croton-chloral have been such as might be anticipated from a consideration of its physiological action; as the primary effect is shown in producing anæsthesia of the head, and only after this is well

marked does its influence extend to other regions, so my experience has been that its remedial value is almost limited to painful affections of the head. My want of success in the treatment of pains in other regions, although at variance with not a few reported results, is in accordance with the experience of Dr. Ringer.

In my hands croton-chloral has been of the greatest use in neuralgias of the fifth nerve, and has appeared in many cases to act as a specific. Its effect is not always, however, to be relied upon, and I have found that certain conditions are more favourable to its success than others; thus, the most marked benefit has attended its use in the neuralgias of young or comparatively young patients, especially in the headaches of anæmic women and girls. In these cases there has been either cure or relief in 86 per cent. of the cases treated. About the climacteric period the success has fallen to about 50 per cent., while in later life there has again been a rise to about 60 per cent. At the climacteric period, bromide of potassium has seemed to be more reliable in its action.

Again, when the headache has occurred in patients with marked hysterical symptoms, the result has not been nearly so favourable; in fact, I have come to look upon the presence of hysteria as making the success of the drug very doubtful.

It has appeared to me that when first given this remedy has only a temporary effect—that is to say, when the medicine is left off the headache will return; but that after a longer course the pain is permanently removed.

It is of course necessary to remember that in giving croton-chloral for the neuralgic headache of anæmia we are treating only one symptom of the anæmic condition, and that we must also direct our attention to the removal of that state. Although, however, I am not aware that this remedy can be considered as a direct tonic, yet it is certain that in many cases patients do improve in their general health while taking it; it is an *indirect* tonic, and acts by the removal of what perhaps more than anything else conduces to anæmia and mal-nutrition—that is, frequent or constant *pain*. Doubtless, *when* the general health has been improved by tonics, the neuralgia will in many cases disappear. But the special advantage of croton-chloral is that it will often quickly remove that which chiefly distresses and wearies the patient; in not a few instances, where tonics have improved the general condition, but have left the headache untouched, croton-chloral has quickly cured this; and a too speedy substitution of tonics for croton-chloral has been followed by a return of the pain. The two kinds of remedies may be advantageously given at the same time, so that the croton-chloral may keep the neuralgia in check, while the tonic attacks the original cause of the trouble.

In conclusion, I cannot but express my belief that in croton-chloral we have a valuable addition to our resources for the relief of a very important class of painful affections; and I hope that the experience of others will be even more favourable to this remedy than my own has been.—*Lancet, Dec.* 2 *and* 9, 1876, *pp.* 776, 814.

25.—CASE OF ACUTE TRAUMATIC TETANUS TREATED SUCCESSFULLY WITH CHLORAL HYPODERMICALLY INJECTED.

By J. H. Salter, Esq.

In the following notes are given the details of a case of acute traumatic tetanus, resulting in recovery under the treatment of repeated hypodermic injections of chloral. Although its appearance in print is somewhat late, it is believed to be the first case treated successfully by this method.

The disease resulted from a wound received during a fit of drunkenness, and followed by exposure to unusual cold, in a subject debilitated by habits of intoxication and low in the standard of intellectual development.

The treatment consisted in wearing out the acute character of the disease by the continued exhibition of the drug; neutralizing the tetanic poison, so to speak, as fast as it was secreted: or, in other words, de-polarising the nervous centres excessively charged by the morbid processes of the disease.

Henry Wade, æt. 38, labourer, single, of Tolleshunt Darcy, Essex, on or about February 1st, 1875, fell down when drunk and received a severe incised wound of the scalp over the junction of the occipital with the parietal bones. He was pumped on to stop the bleeding, the water being very cold. In appearance he was an undersized man, with an epileptic cast of face, and of inferior intellectual powers; moreover, he had become addicted to frequent habits of intoxication.

On February, 14, at 5 p.m., I saw him first. He was twitching a little, complained of pains in the back and body, and stiffness of the jaw, which he attributed to cold. I made him swallow a dose of castor-oil immediately, but with difficulty; at 6.45 p.m. I found him sweating profusely, pulse 76, and weak; severe pains in the muscles of his abdomen, with tetanic spasms, which increased in frequency until the interval was not more than half a minute. At 7.10 p.m. there was well-marked empros- and pleurosthotonos, eyes fixed and staring, and the "risus sardonicus" very noticeable. He constantly turned over from side to side in bed, sweating and twitching violently; his temperature was 92°; pulse 84. He was quite sensible; jaw firmly clenched, and the muscular

tension of the neck and back severe and painful. I determined at once to treat him with chloral, and as he could not swallow, I thought the best method of exhibiting it was by hypodermic injection, which I commenced immediately, with five grains dissolved in five minims of water, in his right arm. We fancied even this quieted him a little, and repeated the dose in a quarter of an hour. At 9.5 p.m. he was very restless, and continually turned over from side to side; the spasms became more violent, the perspiration very profuse, the eyes fixed and staring, nostrils dilated, respiration very distressed, jaw clenched, and muscles of neck and jaw rigid and producing in the face the characteristic ghastly grin. There were only a few seconds of rest between each spasm, which were extremely violent and painful to witness. He was quite sensible, and his sufferings consequently agonizing. I again injected five grains of chloral, which afforded him some relief, and in twenty minutes five grains more. He said it eased him, so that ten minutes later, *i.e.* at 9.45, I gave him another injection of five grains, and this quieted him considerably. All through the night the case was watched most carefully, and we were convinced that the spasms succumbed under the influence of the drug. At this time we gave it only in five-grain doses, repeated at intervals of ten, fifteen, or thirty minutes, according to the necessity.

February 15th.—At 12.15 (midnight) he seemed to be under the influence of the drug; the spasms were certainly much relieved, and there was no ill effect apparent from its frequent administration. On the contrary, relief seemed to be immediate and strongly marked. He had passed only about a pint of urine in twelve hours, but his skin had during that time been streaming with perspiration. At this hour I injected five grains into the arm and eight grains into the thigh, which produced a few snatches of fitful sleep at intervals. After one of these naps (at 1 o'clock), he had a terrible spasm, and complained of having more pain in the muscles of the back and spine. I injected ten grains at once, dividing it between arm and leg, and there was an interval of rest till 1.23. Opisthotonos very marked, so that he touched the bed only with his head and heels, groaning dreadfully with pain. At 2.32 a.m. a severe spasm supervened after a long rest; five grains of chloral injected, after which he had a short doze; pulse 80 and weak; at 2.55 a.m. temp. 19°. At 3.7 p.m. spasms less frequent. Chloral five grains, continued at half-hourly intervals until 9 a.m., when the temperature was 87° and pulse 76; during the spasms the pulse became uncountable, and was always taken in the interval; we noticed in it a perceptible change immediately after the injection of a dose of chloral; it always became fuller and more regular in a marked degree;

probably through relief of the tension of the muscular coats of the arteries. Throughout the day chloral was administered in this manner whenever it seemed to be required, our object being to wear down the violence of the disease by its constant use. He could not swallow, every attempt being attended by excruciating pain and violent tetanic spasms; during the day the bowels were relieved three times, after which for six hours there were only three very severe spasms; again, between 1 o'clock and 10 p.m. the bowels were relieved five times, and he passed water more freely. Between the pains he was much less restless, his temperature 95.4°, pulse 72. At 10.50 p.m. I tried to make him swallow some beef-tea, but it brought on spasms so violently that I was obliged to desist. During the night spasms were very severe, and were treated constantly with chloral in five-grain doses.

February 16th.—Towards morning he got several snatches of sleep, which refreshed him somewhat, but he twitched a great deal, warranting the belief that the violence of the spasms was only kept in temporary subjection by the drug, the effect of which was nevertheless most marked; for when injected during an attack it subsided quickly, and the patient looked forward to the repeated puncturings with pleasure as his only relief from agony. He seemed to get materially weaker, but now and then could swallow small quantities of beef-tea and raw eggs with milk. The bowels and bladder were unmoved for several hours. Opisthotonos continued very marked, and gave rise to intense pain, and the spasms were intermingled with violent tremblings. The tension of the muscles of the neck afforded a capital index of the requirement of the chloral, and it became less soon after each injection. The wound of the occiput was dry and red, and discharged nothing, there was no relaxation of the jaw; the pupils *were dilated*, and at 10 p.m. the pulse was 120, temp. 90·1°. The effect of the chloral was noticeable in thirty seconds after its administration. Had no injection from 10 to 12.10 p.m. during which interval he got a little sleep. At this hour I was sent for, the patient being thought to be much worse. The spasms were frequent and exhausting in their severity, his agony terrible to witness, and his screams audible at some distance; he was sweating profusely and quite sensible; the pulse was very rapid; he was lying curved like a bow on the occiput and heels, never turning or twisting from side to side now, the peculiar grin of his countenance being very ghastly and remarkable, and the muscles of the neck as rigid as a bow-string. I immediately injected 10 grains into his arm and 5 grains into his leg; the pupils were dilated, fixed, and insensible to light—respirations 36. His features were simply satanic. At 12.43 muscular tension was

relieved, but he was exceedingly exhausted, and violent shakings were substituted for tetanic rigors.

February 17th.—At 12.55 (night) he felt in pain everywhere; at 1.15 passed half a pint of urine. Chloral 8 grains. Pupils still dilated and insensible, teeth firmly clenched; dozes at intervals, apparently worn out, but repeatedly disturbed by a spasm, or trembling more or less severe. At 2.11 a.m., temp. 93°, injected 8 grains of chloral; pupils unequal and dilated. At 2.26, he managed to swallow with difficulty, through an opening made in his teeth, half a cup of broth and some raw eggs; tried to turn over on his side, but could not; the "risus sardonicus" still very marked. At 3.15 had rested a few seconds, but signals that he is in great agony; at 3.45, 5 grains of chloral; at 3.59 a.m., temp. 95°, pulse 105, perspiration less. Could not take nourishment so well, and swallowed with greater effort; appeared to have become very weak, and was exceedingly drowsy; spasms continued very severe. At 4.17 a.m. had a short rest of five minutes; complained of great pain in the epigastrium; took a little more beef-tea. At 4.38 a.m. muscles very tense: he complained exceedingly of his back and stomach; chloral 10 grains. At 4.49 a.m., temp. 93°, pupils unequal and widely dilated; 5.15 a.m. somewhat improved; always better after the chloral, which encouraged us to "stick to him." At 6.15 a.m. chloral 5 grains in arm, and same in thigh; pupils acting again. Between 6.15 and 8.5 a.m. had about a dozen spasms, but not so severe, with intervals of rest varying from 3 or 5, to 14 minutes at a time, the latter is the longest yet recorded; pupils more natural in size, and sensible to light; pulse 88; temp. 96.3°. At 9.52 a.m. five grains of chloral in left arm; quieter immediately; passed water freely, and once or twice moved his jaw laterally with great effort; but as this always induced a spasm, he was afraid to venture further.

Throughout the day he was quieter, but seemed very exhausted. At intervals we managed to make him swallow small quantities of nourishment in the shape of beef-tea, raw eggs and milk. His breathing was less hurried, and the muscular tension somewhat relieved. His knees were drawn up in their old fixed position, and during the day he took a little nourishment every twenty minutes, into which a few ounces of sound sherry were mixed for the first time. The wound looked a trifle moist, and the sweating of the skin was much less. He said he felt better, and the "risus" was less horrible to witness; his urine was very offensive. Opisthotonos continued, and he felt very worn out. If it were not for the chloral injections, which were administered, sometimes every half hour, sometimes with an hour or two's interval, the spasms threatened to come

back with all their original intensity; but while the chloral was introduced from time to time it seemed to overcome the tendency of the system to succumb to the disease. From 9.35 to 11.45 a.m. there was no spasm, but twitchings were plentiful. He took nourishment freely—all concentrated. At 3.30 a.m. he separated his teeth with difficulty sufficiently wide to admit the passage of a fourpenny-piece between them; pulse full; sweating very little; chloral given at longer intervals. At 8.20 a.m. pains seemed to increase again, and sweating returned, but after injecting six grains of chloral into the arm and the same into the leg, the urgency of the symptoms subsided. From 9.30 a.m. to 1.25 p.m., only three severe spasms. He was very sleepy; pupils natural; passed urine freely. Chloral, 5 grains, produced great drowsiness at once. Better all day, but unable to do without the chloral longer than 1½ hour, or 2 hours; temperature varying from 94 to 97°; towards night he was more restless; moving upwards and downwards in the bed instead of from side to side, never attempting to turn over, as heretofore; pains shorter but acute, intervals much longer; jaw very rigid; the wound looked better, and discharged some thin pus for the first time, which we looked upon as a favourable omen. At 4.10 he separated his teeth and showed the tip of his tongue; temperature 94° pulse 64; skin moist, not sweating; felt very sleepy; and at 8.45 p.m., immediately after the injection of 5 grains of chloral, he had a quite doze for 20 minutes without a break. Kept working up and down.

February 18th.—12.45 (midnight), had 2¼ hours of quiet sleep, disturbed slightly by occasional twitchings. His muscles were nevertheless very rigid, and he still lay on his head and heels; he perspired freely and looked very haggard, but sensible; pulse quick and feeble; tendons of the hamstring muscles as tight as whipcord. Pillows were placed under his back and knees, from which he experienced relief. If anything the opisthotonos was more severe than formerly, and his breathing very laboured at times. He complained of great thirst, which could not be relieved, except by teaspoonfuls of nourishment frequently repeated. We managed to get down him in the 24 hours about 12 ounces of good sherry, several eggs, milk, and concentrated beef-tea; the wound discharged more healthy pus. He said he felt no better, but weaker. His position was altogether more fixed and rigid, but he had fewer spasms and tremblings. The chloral was repeated at intervals varying from half an hour to two or even three hours, in doses ranging from 5 to 10 grains, in accordance with the intensity of the symptoms, so that he was never left more than a couple of hours, night or day; and I must offer my tribute of warmest praise to Mr. Charles Donkin, my assistant, who volunteered

and assisted me in carrying through during thirty-two days and nights of anxious watching this most trying but interesting case.

February 19th.—At 8.30 a.m. was much quieter, but jerked very much occasionally; pupils sensible to light, but singularly uneven; very drowsy; intellect unimpaired; pulse 80 and better; muscles less rigid; he could sometimes open his teeth so as to admit the top of a feeding spoon; took nourishment in small quantities freely and very frequently; temperature 96°. At 9.45 a.m., the right eye was discharging through eversion of the lower lid; right side of mouth and eyelid drawn down; pulse improving—72; always astonishingly fuller after the chloral injections, which were still alternated between arm and leg; the muscular rigidity of the trunk remained unabated. At 11.7 and 12.15 a.m. not much change; but going on apparently satisfactorily. At 12.30 we administered an enema, per rectum, of soap and water, which returned immediately unstained. At 1.45 a.m. pains increased and muscles became very rigid; pupils widely dilated; wound discharging well; after each injection the pupils got smaller, face rather less drawn on right side, and his teeth could be separated a little more; but he was restless, and his breathing was very laboured; right pupil sometimes larger than left, but varying; very weary, and dozing continually; pulse 84; passed water freely; on the whole the day was much freer from spasm, but he got weaker, and more despondent; chloral administered at intervals of 3 hours throughout the day.

[Mr. Salter goes on reporting this interesting case every day (giving injections of chloral at almost every attack) till March 3rd, and ends the case as follows:—]

March 2nd.—1.30 a.m. Has slept well, and is refreshed; about three spasms since 10.30; chloral, gr. viij. 3 a.m. No spasms; still sleeps; chloral, gr. vi. 6.50 a.m. No spasms; very sleepy; gr. vi.; pulse 92. 7.30 a.m. One slight spasm; very sleepy; chloral, gr. viij.; more comfortable altogether. 9.50 a.m. One spasm, slight; weary of lying; temperature 96·4; pulse 108; chloral, gr. vi. 1 o'clock p.m. One spasm; chloral, gr. vi. 3.15 p.m. None; chloral, gr. vi. 4.55 p.m. Bowels moved, two spasms attending the effort; pulse 104; chloral, gr. viij. 6.25 p.m. One spasm; slight cough; chloral, gr. x. 9.30 p.m. One spasm; cough troublesome; chloral, gr. x.

March 3rd.—12.30 night. One spasm whilst forcing water; chloral, gr. vi.

From this time to March 7th, the chloral was continued at variable but longer intervals of 2, 4, and 6 hours, according to the continuance of the spasms; but after a cough which caused

us some little trouble, accompanied by some congestion of one of his lungs, had been disposed of, all went well, and he regained permanent convalescence slowly but surely, in the course of a few weeks.

October 8th, 1876.—Writing now one year and ten months afterwards, I can state that the man is hale and hearty, and does a good day's work as well as any labourer of his calibre.—*Practitioner, Dec.* 1876, *p.* 420.

26.—ON THE TREATMENT OF CHOREA.

By Dr. J. Magee Finny, Physician to the City of Dublin Hospital.

My object is to bring under notice the advantages of a certain line of treatment, without in the least claiming that it is either new and untried, or that it is the best and only treatment to be adopted in chorea.

It is a line of treatment, however, which, as far as I can judge from observation of the treatment of chorea by other practitioners and the perusal of the cases recorded in our journals, is one that has, of late, rather fallen into disuse and discredit.

Case 1.—*Severe Chorea; no history of Rheumatism; no cardiac complication; Treatment by Sulphate of Strychnia and Ether-spray, commenced a fortnight from the beginning of illness; Immediate benefit; Recovery in fifteen days. Total duration, four weeks and one day.*—Richard F., aged thirteen, came under my care, in the City of Dublin Hospital, 15th May, 1875, a fortnight ill. No cause for the attack could be obtained, except that he was a very studious boy, extremely fond of reading, and used to devote most of his out-of-school hours to study. He was a fair-haired, well-nourished lad, and presented none of the anæmic symptoms so common in choreic patients. Before he came into hospital he had bitten his tongue on several occasions, but, although the movements of his body and extremities became much more excited for some days after admission, the tongue escaped further injury.

This lad was a well-marked example of severe choreic disturbance—for, in addition to the ordinary "madness of the muscles" of the eye-lids, lips, forearms, hands, and legs, he was totally unable to stand or take a single step. His whole body, as he lay in bed, was so jerked and thrown about, that it was found necessary to prevent his falling out by tying a folded sheet across the bed; while any attempt to lift him up in the bed threatened dislocation of the head, so violent and jerking were his nods. His speech was also greatly impaired by the want of co-ordination of the expiratory muscles and those of the tongue and lips,

a sucking inspiration continually interrupting even the shortest sentences. The treatment he had received before I saw him had consisted of succus conii and shower baths, with beef-tea and milk dietary.

On 15th May, I commenced the administration of strychnia in doses of $\frac{1}{60}$th grain of the sulphate three times a day.

On 17th the dose was increased to $\frac{1}{48}$th grain, and each day it was increased little by little.

The prescription I adopted is one recommended by Hammond, which, by its accuracy and the facility of regulating the dose, supersedes that of Trousseau.

A solution of sulph. strychniæ, of half the strength of that of the British Pharmacopœia, is made, so that five minims shall represent $\frac{1}{48}$th grain of the alkaloid. Sufficient is ordered for one day, and syrup added. Each day an additional minim of this solution is added to the dose of the day before, so as gradually to reach the dose which will produce the physiological effects of this powerful excitant, or which will suffice to check the excited movements of the muscles. In the case before us I employed, in addition to the strychnia, what seems to me to be a useful adjunct—namely, the application of the ether spray to the spine, from the nape to the sacrum—a line of treatment, introduced by Zimberlin and Lubilski, which is said of itself to be curative.

On the 17th I first applied it to this boy's back for four minutes. The operation was not very easily carried out, owing to his excited state—increased, doubtless, by the alarm at the novel remedy, so that it became necessary to have him held during its application. Immediately the spray was stopped and his fear passed away; the patient said he felt better, and he could speak with more ease.

It is commonly the case that the first application is dreaded most, and gives rise to most alarm, but afterwards this fear gives way to one of actual pleasure; so it was in this boy, for on the second occasion (21st May) he did not seem to mind it, and on the third and last (the 26th) he said he liked it, and, to all appearances, he enjoyed its application. After each spraying he always expressed himself more clearly and intelligibly, and said he felt steadier and better.

On the 19th and 20th May—that is, in four or five days from the beginning of the treatment when the dose of the strychnia had reached only $\frac{1}{30}$ gr. ter die,—there was marked evidence of improvement. The patient had much more command over the muscles of his hand, so that by an effort (of certainly no long duration) he could keep it steady, and open and shut each finger in succession. He no longer needed the restraint of the folded sheet across him; he could also stand and walk very

fairly with help; but the nodding of his head still persisted, and seemed beyond his control, so that it gave him a curious appearance as he walked. The next day he was able to feed himself; he was allowed up on the 22nd—the seventh day of treatment; and on the 26th he was able to walk out in the grounds of the hospital unsupported, his head being now quite steady. He was also able to hold a book and read it, and, in fact, he spent most of the remaining few days of his stay in hospital in this occupation. He left hospital, seemingly quite well, on the 29th, having been under strychnia treatment fourteen days. The highest dose of strychnia reached in this case was $\frac{1}{17}$th grain on the 27th. It, however, produced none of its physiological effects, such as pains and stiffness in the neck, arching of the back, or cramps and pains in the legs. The only marked effect seemed to be the production of an enormous appetite for bread, of which the patient devoured, in addition to his allowance, an extra medium-sized loaf per diem.

Ten days after leaving hospital he came back with a slight return of the unsteady movements, but these passed away in a few days, and they were very probably induced by his eager haste for study, as I learned he had, immediately on leaving hospital, resumed his studies at a school in Marlborough-street.

The total duration of chorea in this case—excluding the relapse of a few days—was four weeks and a day. Improvement followed four days treatment, and cure was effected in fifteen days.

I am indebted for the notes of the foregoing case to Mr. W. Fraser, my clinical clerk.

Case 2.—*Severe Chorea; primary attack, bilateral; no Rheumatism or Cardiac Disease; Treatment by Strychnia and Ether-spray; Improvement in Four days; Cure in Twenty-five days of Treatment. Total duration, six weeks and three days.*—Catherine T., aged eleven, was admitted into the City of Dublin Hospital on the 5th January, 1875. A week before admission I saw her in the extern department of the hospital, having walked thither. Her symptoms then consisted of grimaces, awkard jerky mode of walking, and dragging, unsteady motion of the left foot; but between that day and the 5th, the choreic movements became much worse, and were so aggravated that she was unable to stand or walk, and had to be brought in a cab to the hospital and carried up stairs. She is the daughter of a man who had a year ago left the army, and seemed to lead a vagabond life. Her mother died two years before, and she has since been living a sort of gipsy life with her father, and by her general appearance, as well as by the presence of two spots of tinea circinata, it is evident the child was much neglected. Eight months before, while in Manchester, she had been ill of

fever, but of what sort could not be ascertained. She had suffered from no fright, nor any specially cruel treatment, nor had she been with any case of chorea. She is a very bright, intelligent-looking child, with dark hair and eyes.

Her father stated that, about a week before I first saw her, he had noticed a "drag" in her left leg in walking, and that her left arm afterwards became unsteady; he complained also that "she kicked so that he could not sleep in the same bed with her."

The symptoms of the disease in all its fantastic shapes were well exemplified in this child, though they did not include biting of the tongue. The upper extremities were more affected than the lower, the left side being the worst, and the most remarkable feature was the rapidity with which complete supination, pronation, and rotation of the arm were performed.

Most careful examinations, frequently made during her stay in hospital, failed to discover any bruit or other evidences of any functional or organic disease of the heart; and although the pulse at times was quick, it seemed attributable to the general muscular excitement. Anæmic symptoms were not prominent.

Treatment consisted of plain nutritious food and the gradual and persistent use of strychniæ sulphas.

On the 9th January, ℥. 4 of the solution of sulph. strychniæ (referred to in last case), equivalent to about $\frac{1}{60}$th grain, were administered in syrup three times a day.

On the 11th it was increased to $\frac{1}{40}$th grain.

On the 12th to $\frac{1}{30}$th grain; and so on each day to the 18th, when 14℥. of the solution, or $\frac{1}{17}$th grain, were employed.

Improvement did not show itself the first two days of the treatment, rather she seemed worse. However, on the fourth day, while taking $\frac{1}{30}$th grain, she began to show signs of amendment, and each day she got better and better. This continued to the 19th, when, as I have said, $\frac{1}{17}$th gr. was reached. On that day it was found necessary to stop the treatment, as the child complained of pain in her neck, with stiffness in neck, back, and legs; and when placed on her feet she was unable to stand, being inclined to arch backwards, the toes of both feet were drawn forcibly in under the soles, and she cried out from the pain. All medicine was omitted, and towards evening these symptoms, indicating a full physiological dose of strychnia, had entirely disappeared—and, what was of equal importance, with them to a great extent the irregular movements. Next day it was most evident to everyone that she was very much quieter than ever before; and, except when spoken to, or on attempting to sit up, the choreic movements had well-nigh ceased. I thought it well, however, not to stop the medicine

altogether, so the patient recommenced next day the sulph. strychniæ in doses of $\frac{1}{24}$th grain.

On this day I applied the ether-spray for two minutes, a longer application serving but to alarm the patient. I employed it again on 22nd and 27th, and it was followed by quietude in each instance. From the 20th January to 1st February the dose of strychnia was gradually again increased, and $\frac{1}{15}$th grain, ter in die, was reached before its physiological effects again showed themselves to a slight degree. The dose was again reduced to $\frac{1}{20}$th, and continued at that dose for four days longer, when pills of ferrum redact. were substituted. The improvement, which first showed itself in so marked a manner on 20th January, never went back, but rather steadily increased and continued without interruption till February 3rd, when *all* the symptoms, which on admission on January 5th she had exhibited, were completely gone. She was able to command the muscles of the face; could keep the arms and forearms steady; could extend and flex at pleasure her fingers; could walk with ease along a chalked line, and go up and down stairs. She left hospital perfectly well, fat and strong, in a few days fterwards.

She was thus under strychnia treatment for twenty-five days, while improvement showed itself in four days; and the whole duration of the attack was but six weeks and three days.

Case 3.—*Primary Severe Chorea, bilateral; no Rheumatism or Cardiac Complication; Treatment, Ether-spray and Nerve-sedatives; Improvement.*—Joseph G., aged nine years, was admitted into hospital in March, 1876, suffering from chorea of about fourteen days' duration. It appeared that about this time his mother noticed strange twitchings of his head. The irregular movements were next observed in the right arm, and afterwards in the right leg. The left side was then affected, but the movements were never so severe in the left as in the right side. There was no history of rheumatism. On admission there was well-marked chorea of the face, eye-lids, lips, and tongue; the whole body, and extremities, particularly those of the right side, were in constant agitation, so that various parts of his body were erythematous and abraded; the child could not stand, but he was able to walk in a wild, spraddling, plunging manner. The uneducated looker-on, in addition to being struck by its ludicrous appearance, must have expected such muscular exertion to be attended with perspiration and subsequent fatigue Though well known, it is, nevertheless, a very curious fact, that sufferers such as this case and the others I have described, although they writhe, wriggle, toss and twist from morning to night, do not betray fatigue, nor are the most violent movements attended with perspiration. The patient could speak

tolerably distinctly, though the words of the sentences he employed were run into each other with wonderful velocity, and many an ill-timed in-sucking of the breath made them halt and stagger in a curious way. There was no evidence of any organic disease of the heart or blood-vessels.

The only cause which could be learned as at all likely to produce chorea, in the absence of his having received any great fright, or of his having witnessed chorea in other children, was the existence of intestinal worms (*ascarides lumbricoides*), twelve of which had been expelled by santonin, by my colleague, Mr. Croly, who had seen the child before admitting him to hospital.

These entozoa have, by many, been considered a sufficient exciting cause of chorea, but why worms should in one child induce such an affection, and in another produce no nervous symptoms—and why, in this child they did not induce this peculiar train of irregular movements sooner—I do not feel myself competent to explain, and I do not care to theorise about it.

While in hospital two more female worms were got rid of by santonin followed by castor oil.

In this case I was anxious to see which of the two remedies I employed in my former cases—the strychnia or the ether-spray—was the more to be relied on, and, accordingly, I gave no strychnia for the first ten days he was in hospital, and only employed the spray to the spine, from the nape to the coccyx. At first the application was for five, and then for ten minutes. It was used every second day. In addition to the ether-spray, which seemed to be always followed by much comparative quietude, it was found necessary to give him sleeping draughts, as for some time before admission to hospital he had not slept, but tossed about all night. The following draught was ordered and repeated nearly every night:—

℞. Potass. bromid., gr. 10; tinct. hyoscyami, ♏ 20; hydrat. chloral, gr. 10; syrupi simpl., ʒ i; aquæ, ℥ i. M.

At the end of the ten days the report was that the excited irregular movements are much lessened, and that he can perform, with some degree of ease, some small action, such as picking up a coin, shutting and opening his hand, but that he cannot walk steadily, nor extend and flex each finger at will. He was improved a little. I could, therefore, say that in this case the ether-spray, while it lessened the severity of the movements for a while, did not act curatively. The treatment was now changed, and strychnia commenced, but I lost sight of the patient soon afterwards, as his mother, seeing him somewhat better, took him home. The contrast of this

case and the others is remarkable, and points to the advantages of the early use of strychnia.

The method of administering this powerful nerve-tonic, which I employed in all my cases, was that recommended by Hammond, and I was guided in the dose more by its influence on the movements of the patient than by the desire to produce its physiological results, as Trousseau would teach.

Besides chorea, I have employed strychnine in other cases in the manner I have described with excellent results—and in one case in particular, the choreiform movements of cerebro-spinal insular sclerosis were kept better under control by its employment than by other nerve-tonics, or nerve-sedatives.

Administered in this way it is, I am convinced, a safe as well as a most useful remedy. It is not difficult to get children to take it, as its bitterness can always be masked by the various syrups of the Pharmacopœia, and it has the twofold effect of improving digestion and increasing appetite, as well as of keeping the bowels regular—matters of no small moment in the treatment of choreic patients.

Hammond states that he "had never seen the slightest ill consequences follow this mode of treatment," and "that he had carried it out in thirty-two cases occurring in children under the age of fifteen, and in three persons of adult years, without a single failure." Such evidence is worthy of all attention, and should be a sufficient answer to those who object to strychnia being used in doses large enough to produce its physiological effects, but I have a higher authority than Hammond on the safety of this medication and its therapeutic powers; for, as you all doubtless recollect, in his last lecture delivered in this hall (Nov. 27th, 1876)—one in which he dwelt on the treatment of brain-disease—Dr. Brown-Séquard gave it as his opinion—an opinion which must carry with it all the weight and force of such an authority—that in many cerebral diseases strychnine is the chief remedy to rely upon, and that to obtain its good results it should be employed, and employed fearlessly, to produce its physiological effects—nay, further, that these effects ought to be kept up, so that the slight tetanic rigidity of the muscles should be maintained for four, five, or six weeks.—*Dublin Jour. of Med. Science, Jan.* 7, 1877, *p.* 31.

27.—ON THE TREATMENT OF CHOREA.

By Dr. W. Howship Dickinson, Physician and Lecturer on Pathology at St. George's Hospital; Senior Physician to the Hospital for Sick Children, London.

The conclusion that chorea is primarily a nervous, and not a

vascular, disorder is consistent with its relationship to other neuroses.

Chorea, then, as far as concerns its individuality as a disease, must be dealt with neurotically, though general is often more to the point than special treatment, as it may be needful to prevent a patient's dying of a disease before we can attempt to cure him of it. In severe and acute cases, where the patient is being worn out by incessant movement and want of sleep, liberal feeding, stimulants, and the means of procuring timely slumber — the bromides, opium, or chloral, — may enable him to tide over a period of mortal peril. Next comes the use of bodily restraint. The violent and erratic movements of chorea appear to be one mode at least by which the exhausting effect of the disease is produced, and the improvement which follows upon their mechanical control suffices to show that some at least of that effect is due to the actual movement, while, perhaps, some may be attributed to the muscular attempt, which the bandage makes futile, but does not prevent. Added to this, restraint is important in preventing the excoriations and sores which the jactitation causes, and which may contribute perceptibly to the typhoid prostration, which is one of the worst phases of the disease. A sufficient measure of controlment may be sometimes obtained by merely tying the feet together and firmly fixing the upper sheet. A more effective arrangement is an embankment of pillows along each side of the bed closely adapted to the patient who lies in the trough between. In extreme cases it may be necessary to fix the limbs with splints. A well-padded splint, such as is used in hip-disease, reaching from the axilla to the ankle, is placed along each side of the body, with the arm bandaged to the outer, and the leg to the inner aspect. The child, excepting that he can still make faces, has little more power of movement than a mummy, and resembles a Swiss baby within its encasement, which can move nothing but the eyes. Anything which causes alarm or distress is to be scrupulously avoided, but the agitation of the limbs is in itself a source of great discomfort, and any gentle means of preventing it is usually acceptable to the patient.

In less severe cases mere rest in bed will do much, and occasionally all. Chorea will almost always improve up to a certain point, sometimes to recovery, under the simple influences of rest and time. These, and now and then a purge, may be all that is needed. A word as to aperients may precede what has to be said touching special modes of treatment. Constipation belongs to several nervous disorders, of which chorea is one; it is perhaps rather a result of the chorea than its cause; nevertheless purging does distinct good, and sometimes is the only medicinal process needed.

Passing now from medicine in general to medicines in particular, I am bound, with regard at least to the acuter forms of the disease, to give the first place to the sulphate of zinc. This is no novelty in practice; what novelty pertains to it is the denial of its use in chorea. I believe I am not to be generally charged with therapeutical credulity, and on this point I should not have ventured to express a confident opinion were it not that I have had more than ordinary opportunities of correcting by experience any errors into which I may have fallen. Many metals — antimony, arsenic, iron, and zinc — markedly influence the disease in question; antimony perhaps controls the jactitation of severe and recent chorea in the most immediate manner, but it must be given largely to be effective, and so used it adds to the prostration of the patient, and sometimes, I believe, is the chief cause of a fatal result.

Zinc stands next in the order of efficiency. To be of use it must be given in large doses. A grain of the sulphate may be given three times a day, or in a very severe case more often, and a grain added to each dose every day until the dose amounts to between fourteen and twenty-six grains. Thus administered and sufficiently diluted it causes no sickness nor any prominent effect but the abatement of the jactitation and grimace. A scruple or rather less is commonly a sufficient dose, but much more may be given. In an exceptionally severe case of which the subject was a girl of seven, I gave with apparent advantage, and certainly without harm, a dose which at last reached forty-five grains three times a day, or 135 grains in the twenty-four hours. Under this the child became able to talk, feed herself, and walk, none of which she could do before. The greater amount passes off by the bowels, and the metal can be recovered from the fæces. I have not succeeded in finding a trace in the urine, so that probably but a small proportion is absorbed; though from the greater effect upon the nervous system of large doses than small, it is probable that the quantity absorbed bears some relation to the quantity swallowed. As touching the curative effect, it may be said that a course of treatment which lasts necessarily for a fortnight secures Time as its ally, in acute disease no unimportant auxiliary. But chorea is a disorder of indefinite duration; the zinc may be begun at any period until the acute form has merged into the chronic, and I have often been able to assure myself that recovery dated from the beginning of the remedy, and not from the beginning of the disease. I have often recognised, as I thought, an early effect of the zinc in a peculiar brightness and clearness of complexion, to be succeeded if the drug be long-continued by marked anæmia. It is hence often advisable to associate with the zinc an unaugmenting dose of sulphate of iron. With the

subsidence of the chorea, the zinc may be gradually withdrawn, and the iron at last continued alone, or with the addition of quinine. Another salt of zinc, the valerianate, is of especial use; it is suited to cases of a less acute type than to require the sulphate, and to those by no means infrequent instances in which the attack has with it some of the characters of hysteria.

Next to the salts of zinc, and often to be preferred to them, come those of iron. Where there is evident anæmia, iron in some shape should be given from the first. Zinc does best with florid children, iron with the pallid; zinc when the symptoms are acute, iron when they are chronic. I have met with good results from the syrup of the bromide; and the valerianate, like that of zinc, may occasionally be resorted to. In the more lasting and slighter forms of the disorder, where perhaps an occasional twitch or grimace or some awkwardness in the limbs is its only sign, arsenic, as a nerve-tonic, in small and long-continued doses, is often of service; and a similar statement may be somewhat more emphatically made with regard to strychnia, particularly if this alkaloid be given together with iron. Thus, for the slighter and more lasting forms of the disorder, the pharmaceutical remedies are iron, arsenic, and strychnia; often iron, together with one of the others. Strychnia, like iron, may be advantageously given as bromide in the liquor strychniæ bromidi.

The smaller shapes or lingering remains of chorea call, as a rule, for general tonics; and among such perhaps the most effective is change of air; there is, indeed, no disorder in which a temporary exchange of town for the country or the sea is more decidedly curative.

Where chorea is much mixed with hysteria, as we sometimes see in developing girls, the treatment must be correspondingly modified. Electricity and shower-baths are sometimes in these circumstances useful adjuncts, though with simple chorea such agitating measures could scarcely fail to be mischievous.

Regulated movements, as drilling or dancing, have been recommended. I have often suggested dancing, and thought it did good, The history, indeed, of the epidemics of dancing mania, which have been credulously traced to the bite of the tarantula, or oddly associated with the name of the Baptist—Herodias appears to have been the means of associating, in mediæval fancy, a profane amusement with the fame of that austere moralist,—supplies many striking illustrations of the influence exerted upon the voluntary muscles by rhythmical sounds; not that the dancing epidemics were what we now know as chorea; they were more allied to hysteria.

I have mentioned only means of treatment which have been found to be, or thought to be, useful. I could make out a

long catalogue of drugs which have been tried and abandoned. Belladonna has been liberally given without effect. The late Dr. Fuller gave to a child with chorea eighty-four grains a day of the extract, of which the purity was ascertained, and the remedy was not destructive either to the disease or the patient. I have used Calabar bean and conia without being able to refer any beneficial result to them; and though I have seen children improve under codeia, I have not been able to assure myself that they would not have done equally well without it.—*Lancet*, *Jan.* 6, 1877, *p.* 3.

28.—TWO CASES OF VIOLENT CHOREA, ILLUSTRATING THE ADMINISTRATION OF CHLORAL HYDRATE IN LARGE DOSES.

By Dr. Robert Bridges.

The lamentable issue of several cases of chorea treated by frequent small or moderate doses of chloral hydrate, and the failure of this use of the drug in other affections where I have had opportunities of observing the facts, led me almost to believe that there must be something specially dangerous in this manner of giving chloral; I fancied too that this effect was due to the effect of chloral on the nervous system when it did not produce sleep, and that it was possible that its action in the sleeping and waking states of the body might be very different, and in consequence of this opinion I determined to use it in such doses that its narcotic power should have a fair trial.

I do not know that chloral may not be specially fit to oppose the nervous disturbance in chorea, but I am sure that it owes much of its value to its power as a narcotic. The disease is naturally suspended during sleep, and therefore a drug which produces sleep is plainly indicated. For by procuring sleep, not only is the actual disease lessened in quantity, but the continuity and habit of it are broken, remission and rest are obtained, with the opportunity for the natural repair and reproduction of worn or injured parts, while time also is gained, in which the malady may die out of itself, or be made to yield to remedies of a tonic nature.

If these considerations are of any weight, it is plain that chloral should be given in chorea in doses that will cause sound sleep: and it must follow the rule for the administration of narcotics at night-time, rather than wait upon the convenience of the physician, who is apt to reckon it as a drug that must be watched in its action, and therefore given by day. In illustration of these remarks I have only two clinical observations to record, but they are both of successful cases. The principle of treatment in each was to administer the chloral so that the

patient should get about ten hours' sound sleep in the twenty-four. With this intent a dose of gr. xxx. was administered, and this would have been repeated, as much, or half as much, or less, according to the circumstances if the patient had not slept. Then immediately on waking a second dose was given, in proportion to the ascertained effect, less or more as the patient had slept well or ill, but in any case less than at first: and on waking again, another dose less than the second, and so on till the amount of sleep required had been obtained, when the chloral was discontinued till the next night. Of the two patients thus treated, one was cured completely in one day: the other had a few choreic movements only left on the third day. In each case I gave a preliminary dose of gr. xv. to discover any idiosyncrasy. The pulse, which was difficult to count, I do not record, because statements concerning its force are liable to inaccuracy: but in neither case was the pulse ever so affected by the chloral as to become feeble.—*Practitioner, March* 1877, *p.* 172.

29.—CLINICAL LECTURE ON TABES DORSALIS, OR LOCOMOTOR ATAXY.

By Dr. J. S. Bristowe, Physician to St. Thomas's Hospital.

I wish to speak of a disease, namely, tabes dorsalis, or locomotor ataxy, which is characterised by distinct lesions of part of the cord. It will be convenient to give you first the details of a case at present under my care in the hospital; in the next place to consider briefly the general symptoms of the disease; and then to enter upon an explanation of its pathology.

The case to which I refer is not a very advanced example of tabes; yet so far as it goes it is fairly characteristic. It is that of a man, thirty-five years of age, who was formerly a custom-house officer, and who appears to have had perfectly good health until a little more than two years ago, when he first complained of symptoms indicative of the nature of the disease which was impending. He noticed then some degree of pain in his back, and at the same time a certain amount of unsteadiness in his gait; his legs were not entirely within his control; moreover they were a little numb. After being two or three months under treatment he says that his symptoms ceased, that he was restored to health, and that he continued well until last September, when the symptoms reappeared. His present condition is as follows:—He has a little difficulty in getting up from the sitting posture; he then has to balance himself with considerable care; and presently, when he begins to walk, you observe

that his movements are what is termed inco-ordinate—that is, are not precisely adapted to the object he has in view. He does not move his legs in the manner in which you and I move ours; but he appears to exert an amount of force which is unnecessary for the movements that he endeavours to execute. He throws his leg violently forward and stamps it down with equal violence. At the same time he is not perfectly steady; there is a tendency to totter. These phenomena are, perhaps, scarcely noticeable when he is actually engaged in walking; that is to say, when you have once started him he continues to walk for a considerable time, and it may even appear to you as if he then walked like any healthy person. But if while he is walking, you ask him to turn, you will see that he does it clumsily; there is then again manifest uncertainty in his footsteps, and a little tendency to lose his balance. If he encounters any obstacle, the same tendency to totter reveals itself. Further, if he were walking in the dusk and along a not perfectly level road you would certainly find him constantly stumbling, and he would, not improbably, fall. Indeed, the inco-ordinate movements would then become much more pronounced than they are at present. He says, besides, what most ataxic patients say, that almost the first indication of his malady was the fact that when walking in the dusk, or going upstairs, or turning corners, there was a difficulty in maintaining equilibrium. The chief factors in the production of the characteristic gait of patients with loco-motor ataxy are difficulty in maintaining equilibrium and the use of unnecessary force in performing the different movements; the difficulty in maintaining equilibrium becomes especially manifest when obstacles present themselves, when the patient is turning a corner, or when he is walking in the dusk, and it is still further increased if the patient closes his eyes. If our patient be made to stand upright with his feet put together and his eyes shut, you will find that he immediately begins to totter, and if he is not held up he speedily tumbles over. This last, indeed, is one of the test indications of the presence of locomotor ataxy: for many patients, in consequence either of exhausting diseases or various forms of paralysis, are exceedingly weak in the lower extremities and have scarcely power to stand, and yet, notwithstanding their weakness, they are able to maintain their equilibrium. But in this case, as in other cases of locomotor ataxy, though the muscles remain strong and fully developed, the maintenance of equilibrium is quite beyond the patient's power, especially, as I have said, if he closes his eyes. I may add that my patient complains of some degree of numbness and tingling in his feet.

Again, he is clumsy in the use of his hands. At first sight it

might strike you on examining him that there was nothing the matter with him in this respect; but if you ask him to unbutton his clothes you will notice a peculiar clumsiness in his actions which is quite unmistakable. If you ask him to take a pen in his hand, he opens his hand wide, and then closes it suddenly upon it; he does not take it up deftly as you or I should do; he does not grasp it with the precision and neatness which we who have the full control of our muscles are able to do. As he walks uncertainly and uses unnecessary force to accomplish feats of that kind, so he uses his hands clumsily, and at the same time employs unnecessary force, opening them widely and closing them violently. Moreover, he has difficulty in so arranging his pen as to put it in the position for writing, and if you make him pass the pen from one hand to the other you find that the same peculiarity attends the movements of the left hand as attends those of the right. If you ask him to perform any delicate manœuvre with his hands when his eyes are closed, or his hands are behind his back, you will notice that the difficulty is greatly increased. You will also find, as in the case of most patients with locomotor ataxy in whom the hands are involved, that he has considerable difficulty in picking up small objects, such as a pin or a needle, from a flat, smooth, hard surface. He has numbness also in his fingers as well as in his feet, but it is limited to the thumb and the fore and middle fingers, the other fingers of each hand being unaffected. That is curious, because, although numbness of the hands commonly accompanies the loss of co-ordinating power, it almost invariably commences in the fourth and fifth fingers. In this case it is the reverse.

I believe I have now pointed out to you all the morbid phenomena which this patient presents; they are limited almost exclusively to the lower extremities and to the hands. He says he has a little difficulty in passing water; but that I believe is connected with some old stricture—at any rate there is no paralytic condition of the bladder, and I think we may safely say that he has perfect control over the bladder and rectum. I have, however, omitted to dwell on one important point—namely, the retention of muscular power in the affected limbs. It is a phenomenon mainly observable in the lower extremities. At first sight perhaps, after seeing him walk, and referring to the description I have given you, you might assume that he was suffering from paralysis, and that his legs were weak; but if you look at his legs you will find that the muscles are fully developed, as fully indeed as those of any healthy man, as fully developed as they have ever been; they have not shrunk in any degree. If you make the patient sit down and flex either leg, and then try to extend it upon the thigh against

his will, you will find that he opposes you with great success. The muscles are not diminished in strength. I do not say that they are increased, but they are not diminished in the slightest degree. This is one of the characteristic features of locomotor ataxy. The muscles, as a rule, until late in the disease, and when from disuse they begin to waste, retain their strength even at a time when the patient is unable to take two steps by himself.

Having now described the symptoms of the case under discussion, I will pass on briefly to consider the symptoms which patients who are suffering from locomotor ataxy generally manifest. They no doubt present, as a rule, the same symptoms as this man; but they are liable to have them in a more aggravated form, and to have other symptoms besides.

I will follow a certain order in the description. First of all, with regard to the impairment of co-ordination in the lower extremities, I think I have said nearly all that can be said upon that subject in the account which I have already given. The degree of inco-ordination present varies of course in different cases, and according to the stage which the disease has attained. In the first instance, as I have already mentioned, the patient is very often attracted to the sense of his condition by discovering that he is unable to walk in the dusk so well as he formerly did, and that he stumbles. That was the case with a woman whom I had a short time since under my care, and whom I expect to see within the next week or so again in the hospital. She has been suffering from locomotor ataxy for eight or nine years. She is the daughter of a farmer, and the first thing she noticed amiss was that in walking about the farm in the dusk she used to stumble and fall. This condition gradually increases upon the patient until he becomes at length utterly unable to walk. In advanced cases he sometimes has great difficulty in rising from his chair, and probably can only do so by placing his hand upon the bed, or upon the back of the chair, or by taking hold of the hands of an attendant; and during this time his legs are probably executing a series of uncontrollable grotesque movements. It may be that, though unable to get up, he is yet able to walk without assistance. In other cases he cannot walk without the aid of a nurse, or a stick, or the back of a chair. In other cases, again, the patient is utterly unable to stand alone, and all the movements of his legs are performed with extraordinary incoherence and violence.

I have already pointed out to you that patients in this condition retain great muscular strength; I have mentioned, I believe, that even in the extreme conditions of the disease, when the patient is utterly unable to walk, and when he has so little control over his legs that he cannot stand without

assistance, you will probably experience the greatest difficulty in flexing or extending either of them against his will. You will find, moreover, that if he plant himself in an upright posture with the aid of a stick he will be able in that position to support a comparatively enormous weight. I think Trousseau adduces the case of a patient who, although incompetent to take two consecutive steps, was yet able, with the assistance of a bed-post, to stand erect, and then to bear two or three students upon his shoulders. Another fact which bears upon the same point is, that patients who are considerably advanced in locomotor ataxy, and who have great difficulty in getting up and in co-ordinating their movements, are often able to walk many miles. A young man under my care walked on one occasion from Brighton to London in a couple of days, and on another occasion sixty miles in three consecutive days; yet he was in a condition that would formerly have been regarded as one of advanced paralysis. Then, with regard to the failure of sensation in the lower extremities. This goes on to some extent concurrently with the loss of power. Sometimes, however, it precedes it, and sometimes it supervenes upon it. It begins in the feet, and gradually mounts upwards, involving sometimes the lower part of the abdomen. It rarely amounts to absolute anæsthesia. More generally it is a feeling of numbness associated with more or less tingling. This impairment of sensation gives to the patient a very peculiar impression with regard to the lower extremities. He feels for example, when he is walking, as if his feet were enveloped in wool, or as if he were treading upon sponges, and in some cases as if he were walking on nothing. The hands become affected secondarily to the legs; and in this case, as well as in the other, the affection begins distally, and then gradually mounts upwards; the fingers first suffer, then the hands, then the arms. The numbness generally first reveals itself in the little and ring fingers, the fingers which are supplied by the ulnar nerve. I suppose this happens in nine cases out of ten, or in a larger proportion than that. Connected with this failure of sensation there is also loss of co-ordinating power in connection with the fingers and hands. These conditions are presented by the patient under my care, as I have already described; there are one or two points, however, with regard to the loss of co-ordination which he scarcely presents, but to which I may call your attention. In a well-marked case the patient does not seem to know, except with the aid of his eyes, the exact position of his hands; and if you make him shut his eyes and stretch out his arms laterally in opposite directions, and then ask him to bring his hands into contact, he probably moves his arms towards one another slowly and uncertainly, then the hands miss one another,

being brought forward in different places, one above the other, or one in front of the other, and it is only after various unsuccessful efforts that they at length meet. The same thing happens if you ask the patient when blindfold to put his finger to his nose; instead of striking his nose he will probably strike his forehead, or his eye, or his mouth.

The phenomena of tabes dorsalis which I have now discussed are the most important and constant. There are others, however, which are by no means infrequent, and which are extremely characteristic when they occur. Patients often suffer from certain affections of the eyes; perhaps the most constant of them is extreme contraction of the pupils. Of course this condition may occur in persons who are in perfect health. Nevertheless it is a curious fact, that in the majority of cases of locomotor ataxy the pupils sooner or later become remarkably reduced in size. Another is a paralytic condition of one or other of the muscles of the eye-ball, inducing an internal or or external squint of one or both eyes. I recollect a patient who was suffering, not severely, from locomotor ataxy, but who at the same time had extremely contracted pupils and a marked divergent squint of the right eye. The case impressed me particularly, because I found that when the patient shut his left eye he was nearly as much at sea as if he had shut both eyes, and was unable to maintain his equilibrium; and, moreover, if asked to walk forwards, which he could then do with the aid of a stick, instead of going directly forwards he would go round towards the side to which he squinted. He did not follow his nose, but his right external rectus. These phenomena may be permanent or temporary; often they come on and disappear again, in other cases they remain permanently. Another affection of the eye consists in loss or impairment of vision; objects are less distinctly seen than formerly, the area of the field of vision becomes contracted, or there is a difficulty in distinguishing colours. These conditions may terminate in absolute blindness. This, which is not very unusual, appears to be connected with atrophy of the optic disc, and probably also with the same kind of lesion in the optic nerve as that which involves the posterior columns of the cord. Sometimes the patients become deaf. I know a gentleman who has been tabetic for the last twenty years, and who ten years ago became stone deaf in both ears. Another affection which these patients occasionally present is some peculiarity of speech. This is not very common, but it occasionally happens; I have seen two or three such patients. The woman to whose case I have already referred, and whom I hope to see again shortly, presents this condition. Such patients are slow in utterance, but they can enunciate every letter and and every combination of letters

distinctly. They will even utter difficult words accurately. But, at the same time, when they speak sentences or converse they slur their words; those which they first utter are perhaps fairly distinct, but before they get to the end of a sentence the syllables become imperfectly pronounced and indistinct. Moreover, it apparently requires a great effort to speak. The woman I have referred to is a characteristic example of this peculiarity; when she talks she opens her mouth widely, and in this and other respects obviously makes great muscular efforts with the organs of speech, just as she does with her legs when she tries to walk, or with her arms when she tries to execute any small operation. I assume that this condition of speech depends upon some impairment of co-ordination of the organs of speech similar to that which is presented by the muscles of the limbs when they are called into voluntary action. Again, the genito-urinary organs present some points of interest. As a rule, patients, however long the disease lasts, retain perfect control over the bladder and rectum; but now and then towards the close of the disease these organs suffer, and the fæces and urine may then escape involuntarily; this, however, is the exception and not the rule. The condition of the sexual functions in tabetic patients is discussed by Trousseau at considerable length. He tells us that some patients in the earlier stages of locomotor ataxy have not only an inordinate desire for sexual intercourse, but an inordinate capability of it; that some will have connection sometimes as often as a dozen times in the night. But this exaltation of function ceases gradually with the progress of the disease, and after a while the patient loses both the desire and the capability of performing the act. The subject is interesting because, as I dare say you know, tabes dorsalis has been supposed to depend upon sexual excesses; that view, however, is not now generally entertained. But it is very likely that it was derived from recognising the excesses to which the existence of the abnormal capacity and desire led so many patients.

There are yet a few other phenomena to which I may devote a word or two. The first is the occurrence of neuralgic pains. These are apt to precede the appearance of the proper tabetic symptoms, and sometimes precede them by many years. The pains of which such patients complain are generally of a shooting character, darting with lightning-like velocity from the back along the lower extremities. Sometimes they occur in single shoots at distant intervals, sometimes in lengthened paroxysms. Occasionally they come on many times in a day, and sometimes they are so much aggravated that the patient suffers from them night and day for considerable periods. They are not always present, however. The patient whom I have now under my

care has, I will not say, never suffered from these pains, but he has certainly experienced them in a very slight degree. Again, they are not always limited to the cutaneous nerves, but occasionally affect the viscera. The patient has severe attacks of gastralgia; or severe pains in the bladder and in the urethra on passing water, or in the rectum, as though it were distended by some foreign body. Affections of the skin are not altogether uncommon. Now and then patients (especially those who are suffering from neuralgic pains) present spots of congestion or some vesicular eruption in the course of the implicated nerves, patches of erythema, or roseola, or herpes, or pemphigus. The woman to whose case I have already referred, during the time at which she was suffering from severe paroxysms in the shoulders and upper extremities, presented an eruption of erythema nodosum occurring almost exclusively in the painful regions. Lastly, I may mention that patients with tabes dorsalis occasionally suffer from certain affections of the joints. These parts, and more especially the knees, hips, and shoulders, are liable to be attacked suddenly. Rapid effusion of fluid takes place into their cavities and into the tissues round about them. There is not necessarily much pain; and, after the affection has continued for a few weeks or longer, it usually subsides, and the patient is restored, so far as the joint is concerned, to health. In some cases, however, the disease goes on to erosion of the joint, and to permanent damage, or, it may be, to dislocation.

I believe I have now described most of the phenomena which are apt to be present in cases of tabes dorsalis; and all of them may be present in certain extremely aggravated cases. I will now add a word or two with regard to the symptoms of invasion. Tabes dorsalis, for the most part, creeps on gradually; much more rarely it breaks out with sudden intensity. Its first indication sometimes consists, as in the case upstairs, in a simple loss of co-ordinating power in the lower extremities; but in a large number of cases other symptoms precede the true tabetic symptoms. For instance, not infrequently the neuralgic pains which I have described occur for months or years before the true symptoms of tabes manifest themselves. You may even be enabled to foretell the supervention of tabes from the characteristic pains which the patient experiences. In other cases affections of the eyes or ears, such as I have described, namely, squinting, double vision, impairment of vision, or a certain amount of deafness, in other cases the various sexual phenomena to which I have alluded, precede for a longer or shorter period the supervention of the specific symptoms connected with the lower extremities.

It now remains for me to describe the morbid anatomy of

the disease, and to explain its pathology. In a lecture which I delivered a fortnight ago I described a disease in which the anterior cornua of the grey matter of the cord were mainly affected. In tabes dorsalis the affection is equally well limited, but it is limited to a different part of the cord, namely, the the posterior columns; or more precisely, to that portion of the posterior columns which lies in contact with the posterior roots of the nerves, and posterior horns of grey matter. It not infrequently happens, indeed, that the posterior pyramids or their continuations downwards are involved in tabes dorsalis, but this is not essential, for in many cases these parts remain healthy. The morbid change is always most advanced in the lumbar region, and, diminishing thence upwards, it may cease towards the upper part of the dorsal region, or in the cervical region. From these statements you will, I think, be able to understand some of the phenomena attending tabes dorsalis. First, as regards the loss of sensation and the neuralgic pains referable to the cutaneous and visceral nerves. It is easy to understand why these should so frequently occur; for although, as you know, the posterior columns of the cord are not themselves concerned in sensation, the parts specially affected in tabes adjoin the posterior cornua and the posterior roots, which are thus concerned; and, as might be supposed, the morbid process is very apt to transgress the boundary between these regions. Next, as regards the loss of co-ordinating power, you know that the posterior white matter of the cord is the channel by which the due combination of movements is affected. You can readily understand, therefore, that if this be affected, and the communication between the supreme motor-centres above, and the muscles to be combined in movements below, be interfered with, inco-ordination of movement will necessarily ensue. There are one or two points of interest in reference to this co-ordinating power. I believe it is generally held by physiologists that the centre of co-ordination for the lower and upper extremities and other parts of the body is the cerebellum, and that (as has been implied above) the destruction of the posterior columns prevents the co-ordinating influence of the cerebellum from being transmitted to the lower part of the cord, and that hence the patient loses the power of regulating his movements. But Dr. Broadbent, in a very admirable lecture, published in the British Medical Journal a few weeks ago, gives a different, and, what I think, is not improbably the correct explanation. He admits that the cerebellum is a co-ordinating centre; but he doubts whether it is the centre which governs the movements of the lower extremities. His explanation of the loss of co-ordinating power in locomotor ataxy is this:—He points out, in the first place (I am now referring only to the lower

extremities), that most of our movements, such as those of walking, depend upon so-called guiding sensations: that when in walking you press your feet upon the ground there is conveyed up by the afferent nerves an impression which, acting upon the sensory portion of the cord in connection with them, is thus transmitted to certain motor nuclei, which are thus ordered or compelled to direct certain movements in the several muscles which they supply. He says further, that when you direct by the will that any movement should be executed by the lower extremities the same kind of thing occurs. You send the order from the brain above to the sensory nuclei below; this order is by them conveyed to the anterior cornua, which, thus directed, regulate the movements which your order requires to be made; and he points out that, supposing the white matter of the posterior columns, which is confessedly the channel along which this co-ordinating influence is conveyed, becomes affected, then whether the guiding sensation is carried from the feet up to the cord, or whether the equivalent impulse to order certain movements to be executed descends from the brain to the same part, that free inter-communication which normally exists between the different nerves and the sensory nuclei in the posterior parts of the grey matter of the cord is interfered with the impulse, instead of being diffused over a certain area, and thus bringing a certain number of muscles into play, acts with all its force upon one or two points; and hence instead of duly regulated movements being performed there arises, in the first place, inco-ordination, and, in the second place, unnecessary violence.—*Medical Examiner*, *Mar.* 1, 1877, *p.* 165.

30.—A CASE OF ACUTE MANIA TREATED WITH LARGE DOSES OF HYOSCYAMIA, DATURINE, ATROPIA, AND ETHYL-ATROPIA.

By Dr. Sydney Ringer, Professor of Therapeutics at University College; and J. S. Bury, Physician's Assistant at University College Hospital.

Dr. Robert Lawson, of the Wakefield Lunatic Asplum, in a valuable paper published in the Practitioner, recommends the employment of large doses of hyoscyamia in various forms of insanity. A case of acute mania being admitted under our care, and after large doses of opium had failed to produce sleep, we determined to try this drug. We also compared the effects of hyoscyamia with those of atropia, daturine, and ethyl-atropia, the latter a new preparation, prepared for Mr. William Murrell and ourselves by Mr. Wright, under the direction of Dr. Graham, Assistant Professor of Chemistry at University College.

We used sulphate of atropia; the crystalline hyoscyamia, prepared in Germany, and the daturine were dissolved with the assistance of sulphuric acid. Ethyl-atropia, being soluble in water, was used without the addition of an acid.

The results in this case fully confirm the strong recommendation of Dr. Robert Lawson, and they prove that atropia and daturine, if not equal, are of nearly equal value to hyoscyamia.

Eliza Reynolds, aged twenty-two, a nurse at University College Hospital, was noticed one night when on duty to be odd in her manner; next day she was unmistakably mad. She lay in bed with her eyes closed, disregardful of everything around her. She talked incoherently, and when asked a question made allusion to a different subject. Her temperature was 99, pulse 104, her breathing irregular. Her bowels were constipated. Beyond her madness we could not discover any disease. Her manner soon changed, for next day she sat up in bed, looking vacantly around her, and continued in this state day and night, getting no sleep in spite of narcotics. Her delusion took a religious form, and she showed a suicidal tendency. She very rapidly lost flesh, her eyes became sunken, and this in spite of her taking a fair amount of milk and beef-tea. Her tongue was always dry, and her temperature normal. She passed everything under her. We felt unless we could give her sleep that she must soon die, and, as morphia had failed, we determined to use hyoscyamia. On the evening of October 25 we gave her a grain of the crystallised alkaloid, with most complete success. We then resolved to give atropia, and see if that would answer as well. We were afraid to give a grain of atropia, so on the first night we gave only a quarter of a grain, but without producing any sleep. We next gave three-quarters of a grain, and then a grain, and afterwards we employed daturine and ethyl-atropia. With sleep the bodily health of the patient greatly improved, but her mind continued in the same feeble state.

The first dose of hyoscyamia flushed her face and hands a deep red, but this effect was not noticed afterwards. Her tongue, as we have said, was always dry before the use of hyoscyamia, and the day after the first dose it was still more so; but subsequently, to our astonishment, the tongue was always moist in the day time, though it became very dry for some hours after each dose of the alkaloid. We were astonished also to find that after the second or third day the skin was always moist during the day; during the early part of this treatment her tongue was very red, though moist and clean, but afterwards it became quite natural. About a week before she left hospital to go home she was allowed to get up, and she constantly walked

slowly about the ward, kneeling down before the other patients. We were unable to take the pulse very frequently, or otherwise very closely study the effects of this large dose of hyoscyamia, as we were afraid lest we should wake her, and we felt that her life depended on her getting sound refreshing sleep. After the first dose of hyoscyamia, the pulse rose from 104 to 144 in half-an-hour; in an hour it had fallen to 120, and then gradually declined, so that after eight or nine hours it was 100 again. The respirations were not hurried. After the second doses the pulse rose from 104 to 125. On the following nights the pulse was very little affected. On the twelfth day of her illness her pulse fell to 80. We may add that throughout her pupils were widely dilated.

Excluding the first two nights with hyoscyamia during which time the patient appeared to grow somewhat accustomed to the drug, and thus it seemed to lose some of its effect, we find that hyoscyamia on an average produced $9\frac{1}{2}$, sulphate of atropia $7\frac{1}{4}$, and daturine $7\frac{1}{4}$ hours' sleep.

It thus appears that in cases of madness or delirium, sulphate of atropia and daturine are as good, or nearly as good, as the far more expensive alkaloid hyoscyamia. Perhaps it may be objected that the sleep she latterly obtained was natural sleep, and not at all due to the alkaloids, but this cannot be true, for it will be seen that on the nights she took a too small dose of sulphate of atropia and on the night she took ethyl-atropia, she had not even five minutes' sleep; the nights were absolutely sleepless; we therefore conclude that the alkaloids did produce the sleep. The sleep was very heavy, but the patient could be awaked, but immediately relapsed into sleep again. Sometimes the breathing was a little stertorous. She slept with her mouth wide open. We have deferred publishing this case for some weeks, that we might learn how the patient progressed after leaving the hospital, as we felt that possibly the alkaloids might cause some of her delirium. We thought that this was improbable, for the delirium of belladonna usually subsides in less than twenty-four hours, and yet in this case, when ethyl-atropia (a drug which does not affect the brain) was given, and she had no sleep, the delirium was always worse the day following the sleepless night.

About a week after reaching home she began to improve, and in a letter, dated December 7, it says "for the last ten or eleven days she has been quite sensible. She is quite cheerful, but her eyes are too weak to read much at present." So we conclude that the effect of the alkaloids on her eyes had not quite disappeared twenty-one days after the last dose of alkaloid.—*Practitioner, March* 1877, *p.* 166.

31.—ON ANÆSTHESIA, AMAUROSIS, AND APHASIA AS A RESULT OF BRAIN DISEASE.

By Dr. C. E. Brown-Sequard, F.R.S., Paris.

[The following are the general conclusions to be drawn from three lectures upon this subject.]

There is certainly, as regards anæsthesia, as regards amaurosis, and as regards aphasia, evidence that any part of the brain can produce those symptoms. On the other hand, there is evidence that great masses of the brain on either side can be destroyed without any marked alteration either in sensibility, in power of sight, or power of speech. There is, therefore, a complete demonstration, by these two series of facts, that the localisations of those various faculties in the brain are to be rejected. And we are led to admit, therefore, in the first place, that if there is any localisation at all—and I admit it fully—it must be of the kind I have mentioned—namely, that those faculties are localised in cells which are scattered: so that, if in an animal, for instance, we take away the brain from the front towards the back, layer by layer, we find that, if loss of sensibility comes on, it is only at a certain moment, when we have taken away a very great part of the brain. So it is with respect to the organ of sight. It is interfered with only to a certain amount, when we reach a certain part in the back. If we begin by taking away part of the back and top, we can go forwards and downwards to a certain point without any marked effect, and then suddenly the symptoms will appear. I need not say that we do not find aphasia, for we cannot, of course, find the loss of a faculty that does not exist. But, as regards the two other faculties of sensibility and sight, we see them disappearing in the way I have said—that is, not gradually and slowly, but, on the contrary, suddenly or rapidly. It seems, therefore, that the nerve-elements endowed with those faculties are spread through and are present in almost every part of the brain.

There is one fact on which I should have dwelt in the two preceding lectures, as it might have rendered clearer what I said. That fact is that, in every case that we study carefully, in which there is disease limited only to a small part of the brain —that is, to those parts which may be considered as the centres or conductors chiefly, or exclusively, of the perceptions in question, and in which there is amaurosis in a complete degree in one eye, or in both, or complete anæsthesia in one half of the body, there is proof that, when only a part of their fibres, or of the cells that serve to the function, is injured, we find that the faculties are lost, notwithstanding the persistence in a normal state of only a part of the organ serving to it. We must, therefore, admit that it is not owing to the loss of function of the

diseased parts that the symptoms appear, and that there must be another mechanism causing the disappearance; and if we take the other fact, that the conductors or centres can be considerably altered without any symptom whatever, we find there also something leading to the view that it cannot be owing to the destruction of the tissue that the appearance of the symptoms, when they do appear, is due, but that there must be some other cause for it. What is that other cause if it be not the irritation? Irritation, as you well know, can produce most wonderful effects. Take, for instance, those admirable facts, asserted by Dr. Bochefontaine, of Paris, showing that every organ in the abdomen and in the chest can be influenced, and influenced immediately, by the galvanisation of a small part of the brain; and take the other facts adduced by the same physiologist, which show so remarkably that the irritation of sensitive nerve-fibres near the brain, but out of it, in the dura mater, can produce movements similiar to those we find when we galvanise the brain itself. If you tickle the dura mater with your finger-nail, the anterior limb will jump just as it would if you galvanised the pretended psycho-motor centres of the arm. If you tickle another part of the dura mater the posterior limb will jump also. So that if we were to conclude as Fritsch and Hitzig, and almost all the physiologists after them have done, we should say that a certain part of the dura mater is the motor centre for the movement of the anterior limbs, and that another part of it is the motor centre for that of the posterior limbs.

Take also an experiment of my own, which is extremely remarkable, and was shown, like many others, to the Biological Society of Paris. To facilitate the operation, anæsthesia was produced by the injection of a dose of chloral into the veins of a dog, on which the upper surface of the right side of the brain was cauterised with an iron heated to white heat. As soon as the animal recovered from anæsthesia he showed all the symptoms of meningo-myelitis in a most intense degree in the hind legs and the lumbar part of the trunk. He was shown to the Biological Society about three or four hours after having recovered from anæsthesia. The symptoms were quite characteristic. My friend and colleague, Dr. Hayem, who has studied meningo-myelitis with great care, exclaimed, "Your dog is attacked with it." I said that an autopsy would show that there was no such inflammation, as I had already made autopsies in nearly similar cases without finding the cord and its membranes inflamed. An autopsy was accordingly made, and no appearance of inflammation was seen—there was congestion, but nothing else—but the cells of the gray matter of the dorso-lumbar enlargement of the spinal cord had evidently been acted on by irritation of the brain, and their activity had been mor-

bidly altered and considerably increased, so as to produce the phenomena observed, which were exactly those we find in genuine cases of meningo-myelitis. In other cases the same burning, instead of producing a morbid activity in the cells of the spinal cord, arrested, or, if you prefer, inhibited the normal activity of those microscopic nervous centres, and produced a slight paralysis—sometimes on the same side, sometimes on the opposite side, but generally more marked on the opposite than on the corresponding side. So that, by those experiments, I found something similar to what we see in cases of brain-disease in man—that is, that an irritation of the surface of the brain can produce two things — namely, engender a morbid activity, or put in play the normal activity of the nerve-cells in the spinal cord, or arrest, destroy, inhibit that normal activity.

These are the two series of symptoms that we find in cases of disease of the brain. We find a morbid activity or increase of the natural activity, and we also find just the reverse—namely, a cessation of activity. If we take the case of cessation of activity, we find that it must depend on something similar to the arrest of activity of the cells of the heart when we galvanise the par vagum. It is very difficult for persons who have not reflected on the point and studied the cases, some of which are favourable to the view in question, while the others have no bearing on it, to admit that the activity of nerve-cells will be stopped, just in the same way as that of the cells of the heart, by an irritation in some part of the brain, so that this arrest of activity will persist for years and years sometimes. But we see, among other facts, which are certainly quite in favour of the view I hold, that sometimes, in cases of disease of the brain, the heart's action itself is considerably diminished or partially stopped for many days consecutively. There are cases on record in which, for seven weeks, the heart has remained beating only between 35 and 45 times in a minute; so that the inhibitory influence exerted on the heart can persist for a very long time indeed; and it seems that inhibitory influences causing paralysis, anæsthesia, amaurosis, and aphasia, can be produced in the same way; and we know that the arrest of the heart's action can stop suddenly in those cases, as, indeed, sometimes happens in those cases of fainting when there is disease of the brain, and when the heart returns to a normal rate of beating or to a state of great activity; and, in the same way, we find that, in rare cases, but, at the same time, very clear and positive ones, which have been well studied by medical men, there has been a sudden return of sensibility, of sight, or of speech. And, as regards sensibility, the fact discovered by my friend, Professor Vulpian—that in cases of organic disease of the brain, it

not rarely happens that galvanising the skin will cause sensibility to return on the side on which it had disappeared—is very much in favour of the view that the power of feeling was only kept in abeyance, was only inhibited, stopped, or arrested by an action which the galvanism destroyed.

Therapeutics.—I do not yet know whether efforts have been to cure aphasia or amaurosis by galvanism. Of course it would be well worth the trial. I have no great hope, however, that such a means will be successful. However, it is desirable that the trial should be made, for the point is one of great importance. The current to be employed should be of that class which acts on the skin with great power, and many shocks should be given in rapid succession. In fact it should be the same kind of current that is found most effective against neuralgia. In making use of the current to obtain a decided action on the skin, as you well know, the terminal part of the conductors must be dry, and the employment of a metallic surface is almost essential.

But it is not by galvanism only that we may have a chance of producing great effects by irritation of the nerves of the periphery, so as to do away with the action—whatever it is—which stops the activity of the nerve-centres. If we employ means such as the actual cautery, in a way I will presently explain, we may have a good chance of doing service; and I have some very good cases showing the effects of that curative agency, having myself frequently employed the actual cautery. The actual cautery may be used so as not to give rise to more than very little pain; and it may be used with more power than if applied in such a way as to give a great deal of pain. Also it may be applied so as not to produce a sore, or to give rise to marks or cicatrices that will last—an important point, if the place of application of the cautery be the face or neck. A brownish tinge of the skin, lasting for five or six weeks at the most, is the only mark that will be made. The instrument should have the shape of one of the points of a small olive. It should be used only when at a white heat, and then passed very rapidly over the skin; and the frictions should be made with some firmness at the proper place.

In cases of brain-disease, the best point of application is the head itself, or the lower part of the neck and the interval between the shoulder-blades. These applications should be made five or six times, but not all at once, for then the instrument would have time to lose the proper temperature. If used after its temperature has sunk from a white to a red heat, it would give rise to great pain and to sores.

Long ago, when I was quite a young man, I studied in the wards of the Charité Hospital, in Paris, the comparative effects

of cauterisation with the white and the red hot iron; and I found that the white, though so little painful as compared with the other, was by far the best.

This application has really far more power in improving the condition of the nervous centres than any other means that I know of. Applications in the place I have indicated are the best, but sometimes we have to make them on the head itself. In fact, in cases of diffused cerebro-meningitis and general paralysis of the insane, a great deal of good can be obtained from it; and indeed, as far as diagnosis can be complete where there is no autopsy, I believe that a patient of mine, a physician in New York, has been cured by this means.

Amongst other means, the use of ice ranks, I think, next to the actual cautery in point of power. It should be applied either by friction or in a pounded form; but I do not recommend Chapman's ice bag, because the action of ice is there too much diminished. The bare ice on the skin or an application of pounded ice in thin linen, for ten or fifteen minutes, on the back of the neck and head, the latter being shaved, is most useful.

Amongst internal means one of the most powerful is strychnine, but it must be pushed to the extent of producing stiffness. If you deal with paralysis, the stiffness must be maintained for a whole month. You must take care to give just the dose that will produce that effect. There is not the least danger in this course, if you watch carefully over the effect produced, modifying the dose according to what you observe. I myself was very timid about it for a long time, and then had no very good results from the use of the remedy. When Professor Fouquier, of Paris, first tried the use of nux vomica in paralysis, and sometimes obtained very great success, he was exceedingly bold, and produced those spasms very violently, but did not mind it. Strychnine, in cases of amaurosis due to brain-disease, when there is no affection of the eye itself, would certainly prove useful, as well as in those other kinds of amaurosis in which its use is so well known to be of great value. I have already found it useful in some of the cases I have attended.—*Dublin Journal of Medical Science, March*, 1877, *p.* 219.

32.—A SUGGESTION CONCERNING THE CONDITION OF THE NERVOUS CENTRES IN MIGRAINE, EPILEPSY, AND OTHER EXPLOSIVE NEUROSES.

By Dr. SYDNEY RINGER, Professor of Therapeutics at University College, London.

In the volume of the Medico-Chirurgical Transactions for 1876 I have written a paper, in conjunction with Mr. William

Murrell, on the true nature of tetanus. We have endeavoured to show by experiment that in the cord and brain there exists a resistive power whereby impressions, conducted by afferent nerves, are restricted to certain areas, and prevented from spreading throughout the cord or brain. We further tried to prove that in tetanus this resistive power in the reflex portion of the cord is diminished or destroyed; hence an impression conducted by an afferent nerve diffuses itself throughout the cord, and nervous force being set free from every part of the reflex portion of the cord, all the muscles become contracted, and instead of a natural co-ordinated reflex action we get tetanus. I give a short *résumé* of our experiments and arguments.

Tetanus is said to be due to an excited or exalted condition of the cord; that strychnia and other tetanising agents stimulate the cord. Now if the cord were merely stimulated we should get increase of natural co-ordinated reflex acts, and not tetanus; but in a paroxysm of tetanus the muscles of the body are all contracted, and it is therefore evident that in tetanus a stimulus excites a discharge of nervous force throughout the reflex region of the cord. Moreover, were the cord merely stimulated in tetanus, as the reflex function of the cord declines tetanus should give way to normal co-ordinated reflex action; but this is not the case, for the tetanic convulsions from strychnia at last grow weaker and weaker, but continue till the animal dies.

Certain poisons, like gelseminum and buxus sempervirens, produce at the same time both weakness of natural co-ordinated reflex action (cord paralysis) and tetanus. Thus, in the case of these drugs, soon after poisoning, the reflex function becomes much weakened, then distinct tetanus occurs. At first we can excite either a co-ordinated or tetanic reflex act according to the strength of the stimulation: a weak stimulus producing co-ordinated reflex action; a strong stimulus, tetanus. The co-ordinated reflex action then grows weaker and weaker, proving the growing weakness of the reflex function of the cord. Next, tetanus declines in strength, but continues till all reflex action is destroyed. In such a case it is impossible that the tetanus should depend on stimulation of the cord; for we have seen that the tetanus was preceded by considerable depression of the cord, and continues till the depression ends in extinction of all cord function. It is obvious, therefore, that tetanus may occur in a cord whose reflex function is greatly weakened, and we must admit that in gelseminum-poisoning an impression or stimulus, say, on the skin, excites contraction of all the muscles because the resistance in the reflex portion of the cord is weakened or destroyed; so that the stimulus diffuses

itself throughout the cord instead of being restricted to a limited portion as in health. Strychnia only paralyses the resistive force, and produces strong tetanus; but gelseminum paralyses both reflex and resistive functions of the cord, yet excites only weak tetanus.

We divided the spinal cord of frogs opposite the occipito-atlantal membrane, and, to prevent bleeding, we pushed a wooden peg through the incision into the cavity of the skull, destroying the brain. In these frogs reflex action continues for several days. We observed that soon after, but sometimes before, co-ordinated reflex action began to decline, on striking the animal between the shoulders the legs were shot out in a tetanic manner; that, as co-ordinated reflex action further declined, tetanus increased and became distinct. At this time, as in the case after poisoning by box or gelseminum, a moderate stimulus, like pinching the toe, excited natural co-ordinated reflex action; but stronger stimulation produced tetanus. The tetanic spasm in some cases lasted from half to three-quarters of a minute, and was so strong that we could hold the animal out horizontally by one leg. The tetanus was always at its height a few hours before all reflex action ceased, so that we could foretell the speedy abolition of reflex action by the onset of strong tetanus. Here it is impossible that the tetanus could depend on stimulation of the cord, for it occurred in a dying cord, and tetanus is most marked shortly before complete extinction of reflex function. The tetanus must be due to diminution or extinction of the resistive power enabling the stimulus to traverse the whole cord. It may perhaps be said that the tetanus depends on the production of meningitis through the operation of dividing the cord—a supposition highly improbable.

So much for our joint experiments. And now I propose to show their application to the theory of nervous diseases.

I venture to suggest that, besides tetanus, other explosive neuroses, like migraine, epilepsy, neuralgia, asthma, &c., are best explained by the supposition that a loss of resistance occurs in certain portions of the central nervous system, whereby impressions conducting to these parts spread beyond their normal area, and produce the various symptoms of these diseases. It will soon be seen that the theory of migraine, epilepsy, &c., that I now advocate differs but slightly from that expounded with so much learning and ability by Dr. Liveing; but though the difference be slight, yet I venture to think that my view embraces and accounts for a large number of phenomena. On page 336 of his classical work on Migraine, he says: "The fundamental cause of all neuroses is to be found, not in any irritation of the visceral or

cutaneous periphery, nor in any disorder or irregularity of the circulation; but in a primary, and often hereditary, vice or morbid disposition of the nervous system itself: this consists in a tendency on the part of the nervous centres to the irregular accumulation and discharge of nerve-force—to disruptive and inco-ordinated action in fact; and the concentration of this tendency in particular localities, or about particular foci, will mainly determine the character of the neurosis in question. The immediate antecedent of an attack is a condition of unstable equilibrium and gradually accumulating tension in the parts of the nervous system more immediately concerned, while the paroxysm itself may be likened to a storm by which this condition is dispersed, and equilibrium for the time restored." At page 390 he says, "The whole group of disorders to which migraine belongs have their foundation in a certain tendency of the nervous system, for the most part innate, and often hereditary, to the irregular accumulation and discharge of nerve-force, which is manifested from time to time in phenomena of a more or less explosive character, whether sensorial, motor, ideational, or organic."

I shall make myself better understood if I apply this theory to the explanation of the phenomena of an attack of migraine. I shall consider first the common form, where the symptoms are supra-orbital headache with nausea or vomiting. In a paroxysm with these symptoms there occurs an evolution of nervous force first in that part of the nucleus of the fifth nerve in connexion with the supra-orbital nerve; the discharge then travels backwards, and involves the centre for vomiting. According to the view now advanced, there is diminished resistance in the nervous structures between these parts, so that a stimulus causing a discharge of force by the fifth nerve can pass backwards and involve the centre for vomiting. It may be urged that, according to this theory, any discharge of force in the nucleus of the fifth nerve should travel to the centre for vomiting, and produce nausea or vomiting; but this is not the case, for if the loss of resistance is slight, only a strong discharge in the fifth nucleus can overstep its usual limits and reach the centre for vomiting. Now, the strength of the discharge depends on the amount of nourishment (potential force) of the nucleus of the fifth, or on the strength of the stimulus converting the potential into kinetic force, chiefly on the amount of potential force. In a paroxysm of migraine, the potential force accumulated by nutrition being discharged, the parts become weakened or depressed, and time is required for nutrition to supply the lost discharged force; and till by nutrition the potential force has accumulated to its original amount, each stimulus conducted to the fifth is limited to the part of the

nucleus naturally associated with it; but when the potential force has accumulated to a sufficient extent, then the discharge overcomes the weakened resistance and reaches the centre for vomiting.

In Dr. Liveing's view there is an excessive accumulation of force; on this view there is no excess of force, but less restraining or limiting power, whereby a normal but strong discharge can spread beyond its natural limits, and induces a discharge in neighbouring parts.

Migraine may be considered a neuralgia of the supraorbital nerve; but diminished resistance is not confined to part of the centre for the fifth nerve, but may extend backwards and involve the centre for vomiting. This view will explain the conditions favouring the attack better, I think, than that supported by Dr. Liveing.

Among the most powerful predisposing causes of a paroxysm of migraine are a depressed state of health, especially of the nervous system, anxiety, and worry. In those liable to these attacks, a mental shock or a slight fright is often sufficient to precipitate a paroxysm. According to the view now advanced, the depressed state of the nervous system still further weakens the resistive force, so that an attack is more readily induced.

It is well known that a repetition of paroxysms of migraine and other neuroses favours the recurrence of the paroxysm. It is difficult to understand how a repetition of attacks should favour an excessive accumulation of force, but each attack will tend still further to lessen the weakened resistive force, and the more rapid the recurrence of the attacks the greater their weakening effect on this force.

This view also explains the periodical occurrence of these attacks. After a paroxysm, the potential force is much exhausted, so that the evolution of force in the depressed area produced by an ordinary stimulus is unable to overcome the weakened resistance; but as soon as there is a sufficient accumulation of potential force, an attack occurs. Thus, the interval between the attacks will depend on the amount of weakening of the resistive power. When, by ill-health or worry or grief, the resistance is much depressed, the attacks will recur frequently—every week, nay even daily. According to this view, we should not expect the interval between the paroxysms to be always exactly the same, for the strength of the resistive power will vary from time to time. Again, when the loss of potential force is almost restored, an unusually strong stimulus will set free sufficient nervous force to overcome the resistance and to extend to other centres. It is well known that in the explosive neuroses, whilst some symptoms are constant, others are very inconstant, being absent with

many patients; even in the same patient they are absent in some attacks, but occur in others. Thus, in migraine, headache and sickness are the most constant symptoms; but, in addition to these, we sometimes get obscuration of sight or spectra or both, also temporary impairment of the mental faculties, perhaps hemiplegia—numbness and tingling of the same side as the headache, affecting the arm or the tongue or even the leg. In some cases the heart is slowed, and in a case under my care the attacks, after several years, caused intermission of the pulse once in about every five beats; the intermission ceasing immediately the attack declined. In another case, when the pain became severe, there occurred a a profuse watery discharge from the nostril of the same side as the headache, and in very severe attacks a slight discharge came also from the other nostril.

In such cases the loss of resistance involves a larger area than when the only symptoms are frontal headache and nausea. The diminution of resistance, however, is not equally great in all the affected parts; thus, it is most marked in the nucleus of fifth and in the structures between it and the centre for vomiting, but less marked in the other affected parts, passing, perhaps, gradually into a normal amount of resistance. In such a case only the severer attacks can spread to the parts endued with only slight loss of resistance; hence in some attacks the symptoms will be fewer than in others. As a rule, the symptoms are most numerous when a longer time than usual has intervened between the attacks, for then the increased interval allows a greater accumulation of potential force; hence, when the attack is excited, a great amount of nervous force is set free, able to overcome the only slightly reduced resistance, and spread further than a milder attack.

Thus, to apply once more this view to an attack of migraine, if the resistive power is reduced over an extended area, then the symptoms will be more numerous, the nature of the symptoms depending on the direction the diminished resistance takes and the extent of area involved; thus, if the diminished resistance extends to the inhibitory centre for the heart, then the pulse is slowed, and even made intermittent; if it reaches part of the vaso-motor centre, then we get those changes in the vessels of the face so well described by the authors quoted by Dr. Liveing; if it reaches the secretory centre for the lachrymal gland, or for the mucous membrane of the nose, these secretions will be increased.

The following is a concise *résumé* of the views just propounded. In the explosive neuroses there is a diminution of resistive power, the nature of the symptoms depending on the part and extent of the nervous system affected; the degree of loss of

resistance varying much in individual cases. When the potential force in the part with diminished resistance is weak, the remaining resistance is sufficient to restrain the evolution of force within its normal limits; hence we get co-ordinated normal action. But when sufficient time has elapsed the accumulation of potential force through nutrition becomes so great that when set free by a stimulus it cannot be restrained by the weakened resistance, but, overstepping its normal boundaries, it involves neighbouring parts, arousing in them irregular inco-ordinated action; in fact, the symptoms of the neurosal attack.—*Lancet, Feb.* 17, 1877, *p.* 228.

33.—A CASE OF PARALYSIS OCCURRING ON THE SAME SIDE AS A LESION OF THE BRAIN, AND ACCOMPANIED BY EPILEPSY.

By Dr. W. Williams, Liverpool.

This history is that of a case of partial left hemiplegia, with epilepsy, following a depressed fracture on the corresponding side of the skull; and in connection with this class of cases, towards which so much interest has lately been drawn by the publications of Dr. Brown-Séquard, the present is peculiarly interesting, from the fact that the paralysis can be justly attributed to the effects of the above lesion causing pressure on the contents of the cranium; for the complete recovery, which subsequently took place, followed immediately the relief of this pressure.

On September 25th, 1873, T. D., a boiler-maker, aged 18 years, was admitted into the Royal Southern Hospital, Liverpool, for a recent compound and depressed fracture on the left side of his skull, about half an inch in extent, and situated on the temporal ridge near the point of its division by the coronal suture, caused through a fall against the sharp corner of some iron-plating. At his admission, there was found to be present considerable concussion, but no well-defined symptom of compression of the brain; so, operative interference not being judged imperative, he was placed in bed, with the adoption of the usual course prescribed for the former state. The partially unconscious state in which he was when first seen lasted till the fourth day, when complete consciousness returned; but it was now noticed that the left upper eyelid drooped considerably, and that there was inability to elevate it completely, with defective sight of the same eye. With these exceptions, however, at this time there was no paralysis or weakness of any member of the body; the patient complaining only of a slight but constant and dull pain at the seat of injury continuing unrelieved, even after the healing of the wound.

Up to January 10th, 1874, nothing of importance occurred; progress towards recovery, it is true, had at that time only advanced to a certain stage; for, although he could go about freely and do any ordinary light work, he was still unable to bear much noise or exposure of the eyes to bright light, and anything approaching violent exercise, or even drinking cold water, gave rise to vertigo. At this date, however, nearly four months after the receipt of the injury, there appeared, and gradually increased, loss of power on the left side; and he became subject to fits of epilepsy, recurring weekly, and characterised by the ordinary signs of a somewhat severe type of the disease—such as foaming at the mouth, biting of the tongue, turgidity of the countenance, short spasmodic expiratory efforts, and violent convulsive movements of the muscles of the face and extremities. During the performance of these latter, the participation of the two sides in the movements was found to be unequal, and much in favour of the left arm and leg; so that, in spite of the fact that epileptic convulsions mostly, if not always, and generally so without apparent cause, are more or less unilateral, in the present instance more than ordinary notice seems drawn to the circumstance, from the association with the paralysis and aura epileptica, that will presently be spoken of, occurring on the same side as the lesion of the skull.

Becoming a confirmed epileptic from January 10th till May 6th, he suffered weekly from persistently recurring paroxysms, each being always preceded by a distinct aura in the form of numbness, commencing in the middle finger of the left hand, but soon extending to the others; and then, on its travelling up the forearm as far as the elbow, a sudden metastatis to the left side of the face would usher in the impending attack. This having lasted a few minutes, would be invariably followed by some somnolency and considerable aggravation of the—since the accident—constant pain at the seat of the injury.

The paralysis, amounting in the case of the leg to the production of but considerable lameness, had, by the date of the operation (May 6th), affected the arm to such a degree that a broad sling had for some time been in use, to afford the necessary support for the all but useless and wasted limb (two inches in circumference above the elbow smaller than the right, which is the smaller of the two at the present time).

In May, it was determined to apply the trephine, with the object of removing the fractured part of the probable evil; and, from its limited extent, this was known to be practicable with a medium-sized instrument. Except some convulsive movements while he was partially under the influence, chloroform was well borne. The piece of skull removed, on examina-

tion at the conclusion of the operation, which was performed by Dr. Nottingham, was seen to be traversed by a fissure involving both the outer and inner tables, and which had permitted an attachment through the medium of a tough membranous slip between the pericranium and dura mater; but the most essential peculiarity was to be found in the presence of a sharp-edged spiculum or disc of bone, projecting obliquely inwards for about the one-eighth of an inch from the inner table, and which must, while the piece remained *in situ*, have impinged upon the dura mater sufficiently to give rise to such irritation of the subjacent cerebral surface as would account for the symptoms detailed, though not sufficient, as a subsequent examination proved, to produce any apparent change in the membrane itself.

In a few weeks after the operation, complete control over the affected limbs was regained; indeed, at the end of a fortnight, the patient expressed his feelings by saying that everything now felt all right. But, although he made a rapid and satisfactory recovery, and although the cause might then be said to have been removed, still the predisposition to epilepsy acquired after the accident did not appear to as quickly vanish; for, besides four attacks, similar to previous ones, occurring soon after the operation, and while the wound was in an irritable state from inflammation, a distinct epileptic fit was experienced six weeks later during a residence in the convalescent hospital, brought on, he fancied, through his having taken a glass of beer. Even now, while feeling perfectly well in every respect, and having had no return of his old complaint since June 18th, 1874—the attack already spoken of as having occurred during his convalescence—and able to follow as well as ever his former employment of boiler-maker, there remains the impression that similar indulgence would yet be capable of developing a paroxysm.

As he was not otherwise predisposed in any way to epilepsy, in the absence of all other known causes, it has to be allowed that the spiculum of bone before referred to indicates strongly the starting point; and if it were the cause of the epilepsy, which disappeared soon after its removal, then also, most probably, it not only was the cause, of the aura epileptica and predominance of action during the paroxysms occurring on the left side, but, in addition, gave rise to the partial paralysis which became located on the same side as the lesion in the brain.—*British Medical Journal, Nov.* 4, 1876, *p.* 586.

DISEASES OF THE ORGANS OF CIRCULATION.

34.—ON THE MECHANISM OF THE SOUNDS OF THE HEART.

By Dr. Arthur Leared, Senior Physician to the Great Northern Hospital.

In a communication to the British Medical Journal of September 30th, Dr. C. J. B. Williams has undertaken to refute my explanation of the causes of the sounds of the heart; and, with regard to his own researches, he says: "I reassert that those observations, made in February 1835, supplied the first complete knowledge of the sounds of the heart, which has given to the profession the means of distinguishing the physical signs in health and in disease." Dr. Williams's researches have undoubtedly been of great value to pathology. His theory of the sounds of the heart stands in the same relation to pathology that certain astronomical theories stand in to the science of astronomy. Although a given theory may not itself be true, certain deductions founded upon it may be true. But, even in the abstract, accurate knowledge is always desirable. Moreover, I have shown that the theory of Dr. Williams does not explain some leading pathological facts, which my theory explains.

Dr. Williams's paper consists of two parts: an attack upon my position and a defence of his own. As the attack comes first, it shall be met in the same order.

A single paragraph from my thesis, "On the Sounds caused by the Circulation of the Blood" (London, 1861), will place before the reader what is essential to be borne in mind in order to understand the controversy. It is this:

"All sounds formed in connection with the circulation are produced by and in the blood itself, and their mechanism is virtually the same. If this statement be true, it would be found, in practice, that a sound of one species would be liable to be changed into a sound of another species. And this is the case; for the normal first sound of the heart is, under certain pathological conditions, converted into a murmur; and on the other hand, a murmur thus produced may again give place to the natural sound. In short, all the sounds may be regarded as modifications of a typical sound, because, under varying circumstances, they are convertible into each other."

"I would object," writes Dr. Williams, "that of all bodies, liquids are the least adapted to produce sound; in contact with air, they become sonorous, and all sorts of noises, rushing, splashing, &c., result, from the trickling of a brook to the roar of the sea or of a cataract; and, in collision with solids, they

cause the noises of wave-dash and rainfall, also musical and other murmurs in the circulation, and the loud notes of the siren and other water-instruments. But, without air or without a solid, it is difficult to make a sonorous vibration in a liquid."

In order to test this matter, and also the effect of viscidity upon any sound produced by the friction of fluid against fluid, I devised a simple experiment, by which it was shown that sounds *are* produced in this way, and that they are modified by the consistency of the fluid. An intermittent column of fluid was forced, by an India-rubber bottle having a tube fitted to it, into a body of the same fluid. By this means, a current sound (murmur) was produced in water, and in a viscid fluid a shock-sound resembling the first sound of the heart. But these sounds are attributed by Dr. Williams to the "vibrating resistance" of the discharging orifice; and he says "the whole idea of noisy collision of fluids, without air or solids, is a mistake." He winds up as follows: "I conclude, then, that the sounds of the heart are *not* produced by any motions or collisions of the blood, because I find that no such sounds can be produced by fluids alone."

Although fully convinced by my own experiments that sounds are caused by the movements of fluids *per se*, it was desirable that this point should be further supported. I, therefore, placed the matter before Dr. Stone, who deservedly stands in the foremost rank of physicists in this country. Here is an extract from his reply: "Any person, whatever his scientific position, who declares the impossibility of sounds being produced in fluid alone, states a fallacy. By fluid, I understand incompressible fluid to be intended, though for accuracy it should have been so stated."

But it would be easy to find further support. Let one extract from a recently published work suffice, as it has a direct bearing upon Dr. Williams's objection—that the sound in my experiment was really due to the vibration of the discharging tube. Speaking of the action of the siren in water, the author says: "The sound which follows proves that liquids enter into direct vibration, like gases, without sound being communicated to them by the vibration of a solid." (A. Guillemin).

In the case of sounds caused by the circulating blood, I do not deny that they may be variously modified by contact with the tissues containing the blood. What I contend is, that they are essentially blood-sounds, and are not caused by contracting muscle, nor by vibration of valves, nor of tubes. As regards vibration, will any one maintain that, when the wind whistles through a keyhole, or when one whistles by blowing into a key, or with his mouth alone, the sound is due in the respective

cases to the vibration of the door, or of the key, or of the lips, and not to the motion of the air itself?

Having shown the fallacy of Dr. Williams's objection, I shall again, but more fully, explain my own views. Many of the facts and arguments which support them must necessarily be omitted.

The normal first sound of the heart is caused by the propulsion of blood out of the ventricles against the opposing blood in the great outlet vessels, provided that three conditions are fulfilled; namely, that the blood is of requisite consistency, that it is of requisite volume, and that it is propelled with sufficient force.

If water instead of blood circulated in the body, the normal shock-like first sound would never be heard. The mobile particles of the fluid would admit of so much friction that a murmur would be the invariable result. But in the case of sufficiently viscid blood, interference with the rapid and intimate intermixture of its previously separated portions modifies the sound. In the so-called anæmic condition of the blood, the first sound of the heart is, for this reason, very frequently found to have degenerated from the normal shock-sound into an abnormal current-sound. But, if the first sound essentially consisted in noises formed by muscular contraction or valvular tension, then in no case would the normal sound be completely absorbed or drowned by a murmur, as it so often is; unless, possibly, in certain rare instances of extremely loud murmur. I maintain that this position is incontrovertible.

The proofs that the two remaining conditions are requisite, must be briefly touched upon. As regards volume, it is well established that, in case of excessive hemorrhage, by which the pressure in the vessels is greatly diminished, the first sound of the heart becomes changed to a murmur. In this case, diminished pressure seems to produce effects equivalent to those produced by insufficient viscidity with normal pressure; that is to say, the blood-particles intermingle so rapidly that a current or friction-sound is produced, instead of a shock-sound. The third condition—sufficient force—virtually applies to every theory of the sounds, and need not detain us.

But, says Dr. Williams: "I have already shown that no sound results from the impaction of one portion of fluid against another; when no air intervenes, the whole movement is one of quiet displacement."

"The case is one of striking through a space at a tight barrier, but of a soft, rather sluggish fluid in close contact." It is implied, in the first of these statements, that the presence of air at the outlets of the heart would contribute to sound. But, as will be shown further on, the very contrary is the fact; for air, in these positions, would annul any sounds formed in

the blood. With regard to the second rather curious statement about a soft sluggish fluid in close contact, striking through a space against a tight barrier, has Dr. Williams forgotten the force with which blood issued from the punctured aorta in his vivisection experiments? The force of the adult left ventricle was estimated by Valentin as sufficient to overcome a pressure of four pounds.

The second sound of the heart is caused by the recoil of the blood in the great vessels during diastole. It is not caused, as held by Dr. Williams and others, by mere vibration of valves; but by a sudden shutting off of the descending blood by valves, surrounded and supported by blood in the ventricles, and by the tissues of the heart itself.

In the production of the normal second sound, the consistency of the blood has no place. Hence, even in the most marked cases of anæmia, in which the first sound is replaced by a murmur, the second sound never undergoes the same change from this cause. Force and pressure in the circulation are requisites, only as affecting the loudness of the sound. No variation of these conditions causes degeneration of the second sound into a murmur. But, as obstruction in any part of the arterial system causes an increase of pressure behind the obstruction, increased loudness of the second sound—accentuation, as it is termed—is taken as an indication of a contracted condition of the minute arteries. In case of mitral valve incompetency, the second sound is changed from a shock to a current sound, on the same principle as when the first sound is changed into a murmur, because of an insufficient volume of blood, as occurs in excessive hemorrhage.

I have introduced, in my thesis, the case of the noise made in a force-pump by the action of the moving water against the water at rest in the discharge-pipe, as an analogy for the first sound of the heart. And I was also the first to point out the close analogy between its second sound and the sound caused by turning a cock at the end of a pipe of some length, through which water is flowing from a cistern. Nothing, indeed, can be clearer than this analogy. For the pressure on the blood, in vessels sufficiently rigid for the purpose, supplies the place of the perpendicular pressure in case of the tube and cock. If a loud sound be produced in one case, owing to the contact of water with metal, a low and comparatively obscure sound might be expected to be formed under fairly similar conditions in the other. But, if it can be conclusively shown that the second sound cannot be caused by mere valvular tension, then there is no reasonable alternative but to accept the tube and cock analogy, as explaining the second sound.

Dr. Williams writes: "The case of the water-pipe and stop-

cock deserves a little further consideration, because its true nature has not been fully explained; and it was supposed, even by Dr. Arnott, to bear on how the heart sounds are produced. If a water-cistern have a stop-cock at its bottom, or connected by only a short tube, no particular sound attends the closing of the cock; but if there be a long descending pipe of several feet in length, the sudden closing of the tap by turning the cock is accompanied by a shock and noise, more marked in proportion to the length and descent of the tube. The cause is obviously as explained by Dr. Arnott—the momentum of the fluid in the long descending tube being suddenly stopped by the solid of the closed cock. This noise is another instance resulting from fluid motion resisted by a solid, and, so far, has a resemblance to the second sound of the heart; but it supplies no proof of the production of sound by a liquid alone, and is quite different from any of the processes of the circulation." The promise implied at the beginning of this paragraph has not been fulfilled. Nothing has been more fully explained. But, with regard to the objection raised, it is impossible to say what exact share the water has and what the cock in forming the sound. The noise is undoubtedly made louder by metallic contact; but if it be admitted that the noise is partly due to the water and partly to the cock, and the analogy be applied to the second sound of the heart, it does not shake my position as to the mechanism of the sound.

By causing a flow of liquid through the heart and aorta removed from the body, I have succeeded in closely imitating the second sound of the organ. But if great precaution be not used in excluding air, so as to prevent it from reaching the aortic valve, even in very small quantity, no sound will be produced. This is because the elasticity of the air breaks the force of contact between the fluid and the valve. But supposing, with Dr. Williams and others, the sound to be caused by the vibration of membranes in fluid, the presence of air would tend to increase the sound instead of destroying it altogether.

Now, Dr. Williams says that he had in his garden, at Cannes, "a water-pipe exhibiting remarkably this phenomenon" of noise on turning its tap. "The noise," he says, "on stopping the tap was not single, but was followed by two or three diminishing repetitions, which I ascribe to the rebound and successive vibrations of the column of water. This implies a slight tendency to a vacuum and momentary separation of the surfaces by gas or vapour, which could take place only in a rigid tube, and never in blood-vessels which are everywhere exposed to atmospheric pressure."

I have shown that the separation of the surfaces by gas or vapour, instead of multiplying the sound, would destroy it

altogether. If the aorta were a metallic tube, the heart-sounds would undoubtedly be very loud sounds; but I deny that the elasticity of vessels or atmospheric pressure are sufficient to prevent the formation of low sounds by the heart in the bodies of animals.

A simple device, which has a direct bearing on what I have just stated, is sometimes employed by plumbers to prevent the noise made by water turned off by a tap. It is to insert one end of a small tube into the pipe, just above the cock, while the other end of the tube is connected with an air-chamber. The concussion between the water and the cock is thus prevented as effectually as it would be by the intervention of a layer of "gas or vapour" between them.

I have devised an experiment, founded on this practice, which circumstances have not allowed me to put into execution. It is this: Having rendered a large animal insensible, to fix in the aorta, close to the valve, a tube fitted at one end with a properly adapted screw-flange, and at the other with an air-chamber. If the result were (what I expect) that both sounds of the heart were extinguished, my explanation of their cause might be regarded as demonstrated.

Dr. Williams recapitulates the observations made in his vivisection experiments; but most of the conclusions drawn from them are not antagonistic to my theory. One only calls for special remark; namely, that whether the heart was empty or full, whether it was moving feebly or forcibly, some (so-called) muscular sound was always present. My explanation of this circumstance, deduced practically from my own experiments, is as follows. When a stethoscope is applied directly to a contracting heart, it is *absolutely impossible* to distinguish the sound caused by the friction of the heart's surface against the stethoscope from any allied sound. The first sound of the heart may, indeed, be closely imitated by movements of the palm of the hand applied to the bell end of a stethoscope, when the other end is applied to the ear.

Instead of attributing the various sounds of the circulation to widely different causes—such as muscular contraction, valvular vibration, and flowing blood (murmurs)—all the sounds, according to my views, are closely related. Under certain conditions, one sound changes into the other; and, therefore, all are formed essentially by the same mechanism.

It results from this, that pathology gives to my theory an amount of support which no other theory can claim. Many phenomena could be adduced in support of this statement, if my limits allowed. I will, therefore, conclude with a question, because, when put on a former occasion, it remained unanswered.

Why is it that, in anæmia, *the first sound of the heart only, and the second sound never*, becomes replaced by a murmur, unless it be that, according to the views stated, the blood loses in this disease one of the conditions necessary for the formation of the normal sound, namely, sufficient viscidity?

If Dr. Williams, or anyone else, will explain this one point, in accordance with any muscular or valvular theory of the first sound, I shall begin to feel doubtful about the conclusions herein set forth.—*British Med. Journal*, *Dec.* 2, 1876, *p.* 707.

DISEASES OF THE ORGANS OF RESPIRATION.

35.—A CASE OF EMPYEMA, COMPLICATED BY PLEURAL FISTULA, TREATED SUCCESSFULLY BY THE ANTISEPTIC METHOD OF LISTER.

By Dr. Edward H. Bennett, Professor of Surgery in the University of Dublin; Surgeon to Sir Patrick Dun's Hospital, &c.

In modern times the chlorides of sodium, mercury, and of lime, and, lastly, the solution of iodine and iodide of potassium, have all been used, mainly with a view to altering the character of the fluid furnished by the pleural cavity by their antiseptic action. The method of treatment adopted in the case which I now report avoids the risks which attend injections of the pleural cavity, and, as its result in this case proves, can attain their object, and lead to a successful issue. I include in the account of this case under the term "method of Lister," the modification of the antiseptic introduced by Thiersch—namely, the substitution of salicylic for carbolic acid, and its combination with it.

A young gentleman, a student of medicine, aged twenty-three, of strong constitution and build, an expert oarsman, suffered the following unfavourable series of illness, all in great part the results of his direct exposure to infection while resident in hospital. First, in 1873, an attack of scarlatina; in May, 1874, typhoid fever; subsequently, in December, again scarlatina, with rheumatic complications; lastly, in May, 1875, a sudden attack of pleuro-pneumonia, which terminated insidiously in empyema of the right side.

On September 15th, 1875, his pleura was tapped with an aspirator, and thirty ounces of sero-purulent fluid were withdrawn. On September 29th, the fluid having again collected, he was again tapped, and fifty ounces of similar fluid were withdrawn.

On 13th October, without any improvement having taken place in the general condition, the right side of the chest being

greatly distended, a superficial abscess was observed pointing over the true ribs in the line of the anterior axillary fold. On the next day this abscess burst; and on the next, the fluid contained in the pleural cavity escaped through the external abscess. Three days afterwards, an attempt was made to introduce a drainage tube into the chest, through the opening of the abscess, but without success. Some inflammatory reaction followed this attempt, and appeared to cause a closure of the passage into the chest. At all events the discharge ceased, and an appearance of pointing at the seat of the second aspirator puncture was noticed. This, however, did not occur, and the matter again, after a few days interval, recommenced to flow at the first opening. From this date until the middle of January, 1876, the fistula, which passed obliquely through the chest-wall upwards and forwards, discharged with occasional interruptions and was horribly fœtid. So intense, at times, was the fœtor, that it penetrated the entire house; and the patient himself devised a means of controlling it to some extent, by receiving the fluid as it flowed in a small tin vessel attached to his side, filled with chlorinated lime. On 20th January, I saw the patient for the first time during this illness, at the request of Dr. Gordon, whom I met in consultation with Drs. M'Dowel and Harvey. We agreed as to the necessity of attempting to control the discharge which the patient's condition—one of advanced hectic—clearly indicated could not be much longer borne. The indications were evidently the establishment of free drainage, and, if possible, a modification of the quantity and quality of the discharge. It was agreed that at first we should attempt this by antiseptic dressing, more particularly as a favourable opportunity seemed to offer for the success of this attempt in the fact that the fistula had almost ceased to discharge for some days, and the odour of decomposition was consequently absent. The treatment was from this date entrusted to me. The right side of the chest was enlarged, measuring fully two inches more in circumference than the opposite; the intercostal depressions were absent; in the neighbourhood of the fistula, which was placed between the fifth and sixth ribs in the line of the anterior axillary fold, the side bulged out excessively. The entire side, except in the infra-clavicular region, was dull on percussion. In this position respiratory murmur could be heard over a small area; and again, along the spine posteriorly, a narrow line could be traced between the scapulæ, between which and the spine on the right side respiratory murmur could be heard. A difference of opinion existed as to the origin of the respiratory murmur heard in this position, equally divided between the views that it was communicated from the sound side, or due to the presence of a

narrow slip of lung capable of respiration, still existing on the diseased side. The result of the case, I think, demonstrates the latter was the correct interpretation. The left lung was resonant throughout, and gave evidence of increased action in the harsh puerility of its murmur. The patient's condition, otherwise, is sufficiently described by the statement that he was suffering from marked and progressive hectic, with considerable cough, but that as yet there did not appear to be any secondary visceral lesion established. The liver was not enlarged, and the bowels tended to be constipated. Troublesome diarrhœa had existed, but was then controlled. The difficulty of breathing, during any exertion, even that of sitting up, was extreme, so that it appeared to be necessary to avoid any course of treatment which might entail any serious disturbance. In the hopes of being able to avail myself of the temporary closure of the fistula, and of the check to decomposition resulting from it, in order to commence the antiseptic treatment, I carefully applied a large carbolised gauze dressing on the morning of the 20th January, and deferred any attempt to open the fistula until later in the day. Late in the afternoon I removed the dressing, and under the spray introduced a tangle tent through the fistula and secured it. The tent was introduced with some difficulty at 9 p.m., and entirely filled the opening, so that no fluid escaped. I selected a late hour in the day to introduce the tent, as I expected that its expansion would give distress, which could be best borne for a sufficient time under the action of the patient's usual evening opiate. Next morning I found that the tent was not causing any annoyance, and that the dressings were not marked by discharge. In the afternoon I changed the dressing, and found that the tent was fully dilated and sufficiently loose in the fistula to allow of its easy extraction. On removing it, while the patient lay horizontally, a considerable flow of matter free of putrescent odour escaped, and a large sized drainage tube was readily introduced. The flow, after the tube was in place, was not nearly as free or as copious as I expected; however, as the patient appeared much distressed by being raised to the sitting posture, and as I was satisfied that the tube was fairly placed in the pleural cavity, I secured it, and completed the antiseptic dressing. Next morning the discharge having escaped from beneath the dressings, I changed them and readjusted the tube. The dressings were flooded with discharge; a sheet folded many times beneath the patient was soaked through, and his body lay in a perfect flood of purulent fluid. This discharge had taken place suddenly in the early morning. I changed the dressings, and found now that the air freely entered the chest cavity while the wound was exposed under the spray. I feared that with so copious a discharge, even with

the dressings changed morning and evening, a failure of the antiseptic process would occur; that in fact all carbolic acid, which the dressings contained in their dependent parts at least, would be washed out by such copious discharges. Having already satisfied myself of the efficiency of salicylic acid as an antiseptic dressing I determined to combine it with the carbolic treatment, with the object of saturating the discharge as it flowed, with a non-volatile antiseptic. In effecting this object it fully answered my purpose. For some days, while the discharge, though diminished, flowed at times so freely as to saturate everything placed beneath the patient, still no odour of decomposition could be detected; and the most remote parts of the folded sheet, which showed stains of the fluid, at the same time readily answered to the test for salicylic acid, the colour of a drop of a dilute solution of perchloride of iron at once changing to purple as it fell upon them. The mode of application of the acid was simply a pad of jute prepared after the method of Thiersch, of three per cent. strength, applied over the wound and drainage tube, and secured by having a Lister's eight-fold gauze and waterproof dressing bandaged outside it.

In three or four days the discharge was greatly diminished, and the patient's general condition appeared much improved. A check in the progress now occurred; irregular rigors, evidently attributable to some complication, not elements of the hectic, which was tolerably regular in its character, affected the patient, along with some ill-defined distress in the pleural cavity. The tube continued to discharge pus regularly, and of unaltered quality. These symptoms were suddenly relieved one day by a large escape of fluid, and at the same time a large area of the chest became suddenly tympanitic on percussion. This area was placed above the level of the fistula, and in front of it. The bulging of the chest-wall which was most marked in this position previously, subsided after this second sudden escape of fluid. I cannot explain these phenomena except by the supposition that a part of the pleural cavity was shut off originally or subsequently to the evacuation of the larger cavity by some accidental septum; that the tension of this cavity gave rise to the irregular constitutional disturbance, and that its relief was the result of the sudden flow of fluid. Whether this be true or not, the patient rapidly mended in health, and the discharge, still large in quantity, became a clear serum, and so continued up to the end of March. Any slight derangement of health altered its quantity—even the introduction of a new drainage tube caused slight increase and slight change in transparency of the fluid, but at last the amount seemed to be nearly constant. The chest-wall had ceased to bulge, and respiratory

murmur became audible over the greater part of the side, extending gradually outwards and forwards from the spine and from below. The area, which I have observed as probably corresponding to the encysted portion of the pleural cavity, retained its tympany, however, unaltered. The larger amount of discharge I now collected from the tube, while the patient sat up and stooped forward. The amount so collected was, for over a month, six to eight drachms at each dressing; this, at the end of March, fell to four, but below this it seemed disinclined to fall. Only a very small quantity stained the dressings. The patient's health at this time was excellent; he had lost all hectic; his appetite was good, and he slept well; confined strictly to his room, and chiefly to bed, for fear of any exposure during the extremely harsh weather of the last spring, he fattened excessively. He could not, however, make any exertion without considerable distress of breathing, and a subsequent increase and alteration of the fluid discharged from the fistula.

During all this time the dressing with carbolic spray, salicylic jute, and carbolic gauze, was carefully continued, and during each dressing the cavity, emptied under the spray, was filled with air drawn in to replace the fluid as it flowed—injected, in fact, with dilute carbolic acid in the state of spray. It occurred to me, now, as the discharge remained obstinately at a fixed quantity, but little escaping into the dressings, and most collected from the tube, that two causes co-operated to delay the progress—the inability of the lung to expand in consequence of its previous compression and adhesions, while the chest-wall, though falling in, was necessarily, from the patient's age and build unable to do so rapidly; and secondly, a condition of inaction of the walls of the cavity, a result commonly observed in the too prolonged use of carbolic acid, or indeed of any other substance applied to granulating surfaces. I met the latter by substituting the spray of salicylic acid for the carbolic, so as to change the application to the pleural surface, for only by the spray in-drawn during the dressings was any substance, except the air filtered through the dressings, brought in contact with the surface. A marked change for the better occurred with the alteration of the spray. The fluid was reduced to four, three, and on 26th March to two drachms, and the dressings were now changed only every second or third day, while the patient moved about his room, and even down stairs. The harsh weather alone prevented me from allowing him to go out. I was prevented from any attempt to remove the tube by the knowledge of the existence of a large cavity still open in the pleura, as proved by the percussion, and by observing the quantity of air that bubbled through the opening at the dressings. The risk attending any attempt to close the fistula by

withdrawing the tube was clearly demonstrated by an accident on the 18th March. The tube, shortly after a change of dressings, while the patient moved about, slipped out of the fistula, and remained out for a day and night, when he felt distress, and sent for me. By this time he was confident that the tube had slipped or become blocked, as he was unable to hear or feel the air passing through it. On its re-introduction a moderate accumulation of fluid—in all about six drachms—flowed, and, as it appeared unaltered in character, I did not anticipate any bad result. However, in the afternoon a violent rigor came on, and he spent a miserable and restless night. The next morning I found him in pain, and looking very ill, with a hot skin. I dressed the wound, but still found no change in the character of the fluid, which was reduced to the normal quantity. I could not lay my hand on the immediate cause of the fever, although its exciting cause was evident enough. The next day the solution of the difficulty was no longer doubtful. The fluid now became purulent, and in quantity again about six drachms. With this an immediate subsidence of the fever and pain showed that the suppuration, the result of the tension, acted as a critical discharge. Now, for the first time, a distinct "leather-creak" sound was developed in the cavity, audible to the patient, and accompanied by a coarse grating vibration, palpable to the hand laid over the chest. These phenomena lasted for two or three weeks, gradually subsiding, and capable of being elicited at last only by forced respiration. In the end of May the patient went out daily, and required dressing only twice a week—in fact, only when the tube became filled by dried secretion.

In the month of June the cavity appeared obliterated, and respiratory murmur could be heard all around the fistula, while the chest wall had fallen in, so as to measure at the level of the fistula two inches less than the opposite side; at the same time the harsh respiration of the sound side had given place to a more normal murmur. On June 20th, after consultation with Dr. Gordon, who constantly watched the progress of the case with me, I removed the tube, directing the patient to keep quiet for a day or two. No disturbance of any kind followed, and the fistula has closed permanently.

In the paper by Dr. Skerrett, to which I have above referred, the treatment by the antiseptic process is contrasted with that by injection of the pleural cavity in the following conclusions.

The advantages of the antiseptic treatment are these:—

1. A free discharge is allowed.

2. Decomposition and consequent absorption of the product of putrefaction are prevented.

3. The treatment is very much less disturbing and exhausitng

to the patient, both because the process itself is much simpler, and also because it is less frequently repeated.

4. There is no irritation of the pleura.

5. One, and perhaps the chief, cause of sudden death during the after-treatment is avoided. In a word, by the application of the antiseptic method to empyema we are enabled to secure all the advantages of a free opening, without any of its disadvantages.

In the same number of the British Medical Journal Mr. Bell advocates "the free incision in empyema *versus* the aspirator," and his cases support the views expressed above as regards the necessity for free discharge as essential to antiseptic treatment. I cannot but think that it is safer to rely on efficient antiseptic dressings rather than trust to free discharge and poulticing combined, with or without occasional antiseptic injections.

Before closing this paper, it remains merely to draw attention to a point in which the case I have recorded presents exceptional features in comparison with the ordinary cases submitted to antiseptic treatment. I found that it was necessary that the air should be allowed to flow freely in and out of the pleural cavity with each act of respiration during the entire period of treatment, a condition not present in any other wound I have treated on this plan, and one which tests to the utmost the powers of the antiseptic used. In the early part of the treatment I applied very wide gauze dressing, secured by a few turns of a broad binder. Subsequently, when the discharge threatened to wash away the carbolic acid by its excessive flow, I added a pad of salicylic jute to provide an antiseptic which should be capable of saturating the discharge as it flowed through and outside the bandages. Subsequently, I gave up the use of carbolic acid spray, to change the character of the application to the pleural surface; and lastly, as the gauze, worn for a long time on the skin outside the pad of jute, caused irritation and severe itching, I substituted for it cotton wool, in the unbroken sheet, saturated with salicylic acid, secured to the side by having ordinary soap plaster spread around the edge of the piece. In this way I was gradually led, as the case progressed, to use only the preparations of salicylic acid in the dressing, and these were constantly worn during the termination of the case, while the patient moved about the house, and lastly walked and drove in the open air. During the entire treatment no trace of decomposition or of fœtor occurred. The pad of jute, prepared after the method of Thiersch, with, on its outside, either a carbolic gauze dressing or an unbroken sheet of cotton wool, charged with salicylic acid, provides a perfect filter for the air necessarily drawn through it into the chest—a

property due largely to the moist condition of the jute, in consequence of its being prepared with glycerine. The original object of the addition of the glycerine in such quantity as to keep the fibres moist (4 oz. to 1 lb. of jute) was to prevent the crystals of the dry acid flying off the jute in dust as it was handled; but I am confident that the glycerine fulfils the further object I have stated, rendering the jute a perfect air filter. A trivial, but still not entirely unimportant point, I have learned also in the management of this case—namely, a ready mode of fixing the drainage tube in such a case, where but one small opening exists, and the risks of escape of the tube either into the cavity or completely out of the opening, as is liable to happen with the constant respiratory and other movements, must be avoided. After various plans had been tried, I adopted, as the readiest and most manageable, a single piece of salmon fishing-gut, carried as a suture through the tube at right angles to its axis, and cut off a couple of inches from the tube at either side. A little piece of isinglass plaster fixes each whisker, as one may name the ends of the gut, to the chest-wall, wet with the spray, and retains it perfectly; the gut is stiff enough to keep the tube adjusted without galling the side, as a wire suture would; and, on the other hand, it does not conduct the discharge to the plaster as silk or thread does, and so prevents its softening and the consequent slipping of the fastenings. The tube was removed and the fistula closed in this case on 27th June last; and a sufficient interval, over three months, has now elapsed to justify me in stating that the cure has been permanent; the patient can walk and ride without distress, and has even put his cure to the test by being upset out of a boat into the sea, without ill effects. His side has fallen in, and the liver has risen considerably in level; but he thinks that his side is again gaining something of its dimensions, more even than is due merely to recovery of power in the muscles; and the respiratory murmur has returned, even beneath the scar of the fistula.

Dr. Gordon has favoured me by reading the proof of this paper, and kindly furnishes me with the following confirmation of its details and statement of the patient's present condition:—

"I have perused the above case very carefully, and can testify to the accuracy of the details and the efficacy of the treatment. The patient was under my care for a very considerable time, labouring under a pleuro-pneumonia, followed by extensive effusion; and, although I advised the introduction of a tube into the pleura in September, the difficulties interposed were so many that the patient was in almost a hopeless state when Dr. Bennett saw him in January. I met him casually at the house

of a relation about a fortnight since, and can testify to his being now in the most perfect health.—S. GORDON."—*Dublin Journal of Medical Science, Nov.* 1876, *p.* 361.

36.—ON THE TREATMENT OF PLEURITIC EFFUSION.

By Dr. F. DE HAVILLAND HALL, Casualty Physician, St. Bartholomew's Hospital.

As I do not intend to allude to the treatment of acute pleurisy in this paper. I will pass at once to the method I employ in pleuritic effusion.

In an ordinary case of pleurisy with effusion, when there exist none of the indications for thoracentesis to be hereafter mentioned, I always begin with the following treatment, if the symptoms be at all acute, and the temperature above 99° F. I order the patient to bed, and even in cases of latent pleurisy where there is little or no febrile reaction, quiet should be enjoined, as the process of absorption goes on so much more readily when the patient is at rest. After attending to the bowels, and securing a daily evacuation by means of a purgative if necessary—and if one be required, calomel or blue pill in combination with the extract of colocynth is the best—I then proceed to the more specific treatment, which consists in keeping the affected side thoroughly saturated with a weak iodine solution. I generally use one part of the tincture of iodine to three of water painted all over the side of the chest from the apex to the floating ribs, and I have a half jacket of flannel made and worn continuously, so that this becomes impregnated with the iodine, and helps to promote absorption. The internal remedies in which I put the greatest confidence are a combination of iodide of potassium and syrupus ferri iodidi, together with the use of cod-liver oil in debilitated subjects, and where it can be taken without upsetting the digestion; but in cases where the temperature is high, it cannot as a rule be borne. Blisters I do not advocate for universal application, but occasionally a blister will give a stimulus to absorption when that process is going on slowly, so that it is certainly worth while to try this form of counter-irritation in these cases. The blister should not be kept open, but allowed to heal as quickly as possible, as the good it effects takes place during the healing process. I have recently had a patient under my care in the Westminster Hospital in whom rest and the application of two blisters had the effect of removing a very large pleural effusion, the only medicine he had being a little liq. ammon. acetatis.

If the urine be small in quantity and high-coloured, a prescription containing tinct. digitalis, potass. iodidum, potass.

acetas, and spiritus ætheris nitrosi has proved very useful in my hands

Dr. Fuller advises the use of the following solution externally: ℞. Hydr. perchlor., gr. iv.; tr. iodi, ʒ vi.—ʒ j.; glycerini, ℥ iij.; aquæ dest, ℥ iv., M. ft. lotio. Or as an ointment: ℞. Hydr. perchlor., gr. iv.-v.; ung. iodi, ʒ iv.-vi.; adipis, ʒ iv.-ʒ j. ft. ungm. And as a diuretic he gives a pill mass made up of digitalis, squills, and the pilula hydrargyri. This combination has met with the approval of many celebrated physicians. Dr. Matthew Baillie, in speaking of pleural effusion, says: "The medicine which I have found most beneficial has been mercury, combined with squills and digitalis. Five grains of the pilula hydrargyri, combined with one grain of the dried powder of squills and half a grain of the dried powder of digitalis, given twice or thrice a day, have in many cases under my care either very much mitigated or for a time removed the disease. There has been some advantage from the mercury affecting slightly the salivary glands. Squills and digitalis are by themselves much less efficacious than when combined with mercury." Sir Thomas Watson mentions this pill with approval, but advises less mercury—one to three grains of the blue pill, with the quantity of digitalis and squills stated above. But whatever treatment be adopted, the alliterative advice said to have been given by a celebrated living physician, of *beef and beer* being the best absorbents, requires to be followed; as, unless the patient's strength be maintained by good food and tonics, degenerative changes are apt to ensue, and a simple serous effusion become purulent.

This plan of treatment will be successful in a large proportion of the cases brought under one's notice; but in a certain number there comes a time when the question of thoracentesis is mooted. Dr. Fuller advises perseverance in the remedies to promote absorption, so long as the breathing is not seriously embarrassed and the general health does not decline; but for my own part, I am inclined to go farther, and say that if, after giving these remedies a fair trial—and I consider three weeks to be ample time—there were then no signs of absorption to any marked extent, I would advise the performance of thoracentesis; because, if done with the proper precautions, it is not a risky operation, and it is hardly fair to the patient to allow him to undergo a protracted illness, with the probability of imperfect recovery and a deformed chest, whereas, by accepting the responsibility of advising the operation, the physician would, in the vast majority of cases, have the satisfaction of seeing his patient rapidly gain ground.

Sir Thomas Watson is opposed to the operation in cases of serous effusion. He remarks: "In simple pleurisy it ought

never, in my judgment, to be performed, unless the life of the patient is, or seems to be, in jeopardy from the continual presence of the liquid within the thorax." But in an addendum to his article on Pleurisy he says, "The operation seems more extensively applicable than I had formerly supposed;" though his opinion as regards its use in simple pleurisy remains unchanged.

There are two sets of cases in which thoracentesis is required in pleurisy, when the effusion is believed to be serous:—

1. Those cases in which the effusion is so great as to threaten to be the immediate cause of death.

2. Cases in which the pleura seems unable to absorb the fluid, and the operation is undertaken to prevent the lung from being irretrievably bound down by adhesions.

As regards the first set of cases, the older physicians seemed to attach only a small degree of danger to an attack of pleurisy; and Dr. Louis went so far as to lay down a law that pleurisy is never an immediate cause of death. That this is far from being true, the instances narrated by Trousseau are quite sufficient to demonstrate, as he says, that "notwithstanding the famous law of Louis, it is possible to die, and to die suddenly, from acute pleuritic effusion." The cause of the fatal termination has not yet been clearly made out; in some cases it is undoubtedly due to the dislocation of the heart so interfering with the proper performance of its functions as to cause sudden syncope. Dr. Evans, in criticising the theory of MM. Blachez and Marrotte, that the obliteration of the pulmonary artery is the cause of sudden death in pleuritic effusion, suggests that the coagula thus formed in the pulmonary artery or its branches may be the cause of the imperfect recoveries so commonly met with in effusions of long standing.

Trousseau gives a most emphatic warning against trusting to the absence of oppression in breathing as an indication that there is no urgent necessity for operating; he says that "oppression is one of the most deceitful of signs," and goes on to say, "It is from auscultation, and still more from percussion, that we must derive our most positive indications as to the opportune moment for performing paracentesis of the chest." I have only notes of two cases in which paracentesis was done when the effusion was serous, and for the relief of dyspnœa. The first one has been reported by Dr. Wardell in his paper on Pleural Effusion; in this case, "63 ounces of clear greenish straw-coloured serum were drawn off to the patient's instant and great relief. That evening she could lie on the right side, which she had not been able to do for several weeks. For some days she appeared to progress favourably; she then, without apparent cause, became worse, and gradually sank on April

23rd. Inspection was not allowed." The other case I have already alluded to when discussing the subject of mensuration. This patient had left pleurisy, for which he was twice tapped; the first time 70 ounces of straw-coloured serum were evacuated, and nine days later 5 pints of clear straw-coloured fluid were let out. On each occasion the patient was suffering from great dyspnœa, and appeared in a very precarious condition; the relief he experienced was immediate, and the temperature fell. This is a typical instance of the good effect of paracentesis in serous effusion, as the patient eventually made a perfect recovery. One of the most usual of objections, and which would if true be a grave one, is that thoracentesis in a case of serous effusion, by admitting air, is likely to cause suppuration to take place in the pleural cavity; but the case I have just narrated shows that this is not necessarily so, and it would be easy enough to collect innumerable instances in which a chest full of serum has been tapped more than once without altering the nature of the secretion; in fact, I am prepared to go farther, and say that it is possible to convert purulent effusion into a serous one, and a case recorded in the Lancet for December 6, 1873, will support this view. The notes are so interesting, and the treatment was so successful, that I venture to give an abstract.

Dieulafoy's aspirator was used in the first instance, and 106 ounces of thick inodorous pus were drawn off, and though there were no signs of the cavity becoming emptied, the operation was now stopped, as a violent fit of coughing came on. In a few days the fluid re-accumulated, and an incision was therefore made close to the former puncture, under a spray of carbolic acid. At least 10 pints of pus, of the same character as before, were evacuated. When the flow had diminished sufficiently, a broad piece of drainage tubing, about 4 inches, was inserted into the cavity, and a very large dressing of muslin and waterproofing applied over the opening, and the patient was loosely surrounded with cotton wadding prepared with carbolic acid, in order to prevent the putrefaction of the discharge. There was a very profuse discharge during the first forty-eight hours, but no decomposition occurred; the quantity rapidly decreased, its character changed, so that on the sixth morning it was entirely serous. On the eleventh day the discharge had ceased, and the opening into the pleural cavity closed up. About three weeks later the patient caught cold, and effusion into the pleural cavity again occurred, which proved to be of a serous nature, and was twice drawn off by Dieulafoy's aspirator. The patient eventually made a perfect recovery.

In the second set of cases in which thoracentesis is required, the only *raison d'être* of the operation is that it should be done comparatively early, as "every day that passes increases the

liability of the lung to become bound down to the medias tinum by false membranes."

I think that I may say, without fear of contradiction, that the modern school of medicine are unanimously agreed that thoracentesis should be performed immediately that the presence of pus in the pleural cavity is suspected, and, as I mentioned in last year's Reports, the suggestion made by Dr. Ringer to use the ordinary morphia hypodermic syringe to clinch the diagnosis where the general symptoms and physical signs have been somewhat dubious, is most useful, and a great improvement on the grooved needle, as originally suggested by the late Dr. Thomas Davies.

Mr. John Wood gives the following directions for thoracentesis; "Tap in fifth or sixth space under the arm just above the rib, in order to avoid any projection from the rib and the intercostal artery. If you go to the lowest limit of dulness, you may perforate liver or diaphragm. First make an incision, draw the skin down, then introduce your instrument."

Trousseau advises "the sixth or seventh intercostal space, nearly 4 or 5 centimetres external to the outer edge of the pectoralis major."

It is better, however, to be guided to a great extent in the choice of the site for the operation by the physical signs as revealed by auscultation and percussion, and I do not see the slightest occasion for over-anxiety to tap at the lowest level of the fluid. Dr. Handfield Jones says, "I certainly prefer to find no breath-sounds in the spot where I am about to plunge my trocar;" but he goes on to say that weak and distant breathing need not deter the operator, as lung-sounds can penetrate through a notable thickness of fluid.

Whether the opening should be closed or left open.—The rules which I have laid down for myself as regards this question are as follow:—

(*a.*) Whenever the fluid is serous or sero-sanguineous, and so long as it continues such, I close up the opening, so as to prevent the entrance of air; and the best way to effect this is with carbolic plaster.

(*b.*) If laudable pus be evacuated, I close the opening on the first occasion, in the hope that the little left behind may become absorbed; and even a second time I would try the effect of sealing up the orifice, provided the pus continued laudable, and only a small quantity had reaccumulated since the preceding operation. Should these two tappings not be followed by a cure, then there remains nothing but keeping the openings patent, and this must be done in any case whenever the pus is fœtid.

If left open, by what means should the opening be kept patent?

This is the question of all others in which I am most interested, as I feel certain that if the practice of making a counter-opening, passing a drainage-tube through, and tying the ends together externally, was more generally adopted, much greater success would attend the treatment of empyemata. "Where only one opening is present, the pleural cavity may be likened to a barrel without a counter-vent, and the escape of the fluid must be irregular, and only partial." The plan of "drainage," as introduced by Chassaignac for the healing of sinuses, consists in passing an india-rubber tube of the diameter of the sixth of an inch, perforated at intervals, throught the cavity, so that the pus is able to escape as soon as formed through the perforations.

Mr. Campbell de Morgan, in an addendum to the article from which I have just quoted, gives a very clear description of the manner of introducing the drainage-tube. A firm, long iron probe, somewhat bent (for children I find a steel sound very useful), is passed through the first opening, and directed towards the back of the cavity at the most depending part. As soon as the point of the probe can be felt, an incision must be made down to the probe, which is then to be brought through the opening thus made. The drainage-tube is then attached to the eye of the probe, and drawn through the two openings, and the ends of the tube are to be tied together, which completes the operation. As regards the after-treatment, all that requires to be done is to envelop the affected side of the chest in picked oakum, which prevents any putrefactive change taking place in the pus after it has flowed from the cavity. Should the oakum irritate the orifices, a small piece of lint steeped in carbolic oil may be interposed at these places. It is astonishing how soon this simple mode of treatment suffices to effect a cure. The last patient I have had under my charge in whom I employed it, had absolutely no discharge on the second day after the operation, though upwards of a pint of pus had been evacuated at the thoracentesis; a few days later there was a little oozing, but this soon ceased, and the tube was withdrawn eighteen days after the operation. Dr. Peitavy records two cases of empyema treated by resection of a portion of the rib, for the purpose of permanently widening the aperture, and so facilitating both the discharge of pus and the injection of fluids. I look upon this as an unnecessarily severe operation, and not required if the drainage-tube be resorted to.

In the able article by Dr. Goodfellow, from which I have already quoted, the history of a case of empyema of five years' standing is recorded. The discharge was profuse, and very fœtid; a drainage-tube was introduced, and three months after the operation the discharge scarcely amounted to 2 or 3 drachms

in the twenty-four hours; and the patient was able to walk in the garden, after being confined to bed for five years. In the other case described in this paper, the counter-opening was not made sufficiently low, so that the matter remained in the chest, became decomposed and extremely offensive. A second counter-opening was made as low down as the probe could be felt through the thoracic wall, and the pus soon lost its offensive odour, and rapidly diminished in quantity. This case affords a very good illustration of the importance of selecting the lowest point for the counter-opening.

Are astringent or other injections indicated?

In answer to this question, I may say that I have been unable to satisfy myself that I have seen much good result from this plan of treatment; and it is one which, if the advice about the drainage-tube be followed, is hardly necessary, as it is only in cases of fistulous empyema with a single opening, and that not at the most depending part of the chest, that there is likely to be any need for the employment of these injections.

If they are used, a dilute solution of liq. potass. permang. (℥ss. ad aquæ 0j.) is about the mildest. Carbolic acid (glycerini acid. carbolici ʒ v. ad aquæ 0j.) is one of the most useful; and a weak solution of the tincture of iodine is said to be very useful in cases in which the pleurisy is of a tubercular or strumous origin.

Besides these three drugs, the whole armentarium of astringent remedies has been exhausted in trying to check the suppuration. The only use I find for injections is to remove fœtor in cases where it is present; but a free counter-opening soon supersedes the necessity for washing out the pleural cavity. Should the drainage-tube, however, be not sufficient to prevent the continual formation of pus, astringent injections ought certainly to be tried; and in the event of these failing, a fair trial might be given to Dr. Dubono's plan of treatment by tannic acid.

His communication records the result of the treatment of eleven severe cases of fistulous empyema by tannic acid, given in a pill mass with the confection of roses, 10 to 25 grains per diem. In eight cases the treatment was most successful, in one there was partial success, and two died. One-half the pills to be given an hour before breakfast, and the other an hour before dinner.—*St. Bartholomew's Hospital Reports, vol.* xii., 1876, *p.* 75.

DISEASES OF THE ORGANS OF DIGESTION.

37.—ON THE TREATMENT OF INTESTINAL OBSTRUCTION.

By George H. B. Macleod, F.R.S.E., Regius Professor of Surgery in the University of Glasgow.

The treatment of a case of intestinal obstruction will, of course, depend on the cause. In any case, if inflammation arise, it must be subdued by opium, ice to the abdomen, or fomentations. Opium is, of course, the sheet anchor. To the adult it is best administered in pill, and to the child in Dover's powder. It must be used freely in any case in which the attack is violent. It may be combined with calomel. Belladonna is now comparatively little used; but formerly it was frequently employed both internally and externally, alone or with calomel. Atropia, too, which was recommended strongly by Dr. Fleming of Birmingham, is little heard of in our day. If leeches be used, they should be applied in such numbers as to produce a decided effect; but they are rarely employed, from the desire to husband the patient's strength. Counterirritation may possibly be beneficial, in the later stages. Ice, by its power of calming irritation and spasms, is of much use. It may also possibly aid the passage of gas by causing intestinal contractions. Grisolle speaks with commendation of cold drinks and cold enemata in these cases.

In the great majority of cases, purgatives must be wholly avoided, and enemata alone used—a principle which is constantly and grossly overlooked in most of the cases one is called to see. When given early, in most instances, purgatives only augment the already exaggerated peristalsis; and, if administered late, they have an exhausted and paralysed bowel to deal with. How frequently, however, is it the case that the whole list of purgatives have been administered—each succeeding dose surpassing in strength that which preceded it; and if at length a favourable change do take place, and the obstruction cease, the most exhausting diarrhœa follows from the combined action of the drugs employed. When, however, a careful examination fails to show that any organic obstruction exists, and there is otherwise no objection to the practice, the exhibition of from ten to twenty grains of calomel in one dose—repeated, if necessitated by its rejection—often works miracles. Of this, I have seen a few remarkable instances, and two will be afterwards related. Galvanism has been tried in cases of obstruction, occasionally with good effects. It is when stercoraceous masses occasion it, that this mode of treatment is of most service. In 1825, Leroy d'Etiolles recommended the current to be passed from the mouth to the anus; but Duchenne proposed that one

pole be inserted into the rectum and the other be moved over the surface of the abdomen, according to the place of suspected stoppage in each case. Personally, I have had no experience of its employment. Even in cases of obstruction by foreign bodies, the use of purgatives is reprehensible. It is now well known by utterers of false coins who swallow their base counterfeits when detected, that a system the very reverse of purgation best rids them of their burden. They keep their bowels confined and distended by bulky and costive food, so as to envelope the coins and allow them to be slowly carried downwards.

If the bowels be much distended with air, they may by punctured, with much advantage. By percussion, we easily make out the best spot for the insertion of the small trocar and canula; and then, if gentle pressure be made on the abdomen, both air and fluid may be made to escape. As the distension goes down, the tube must be gently pushed on to prevent it escaping from the portion of gut it has entered. There is no fear of undue inflammation or extravasation, as adhesions soon form even when it is thought desirable to leave the canula in place. Mr. Platt (see Lancet, January 11th, 1873), in a very curious case of obstruction, removed no less than forty ounces of fluid fæces through a small tube, by using a probe as a piston. Doubtless, in many cases, the aspirator would be found very useful in unburdening the bowel, and so diminishing congestion and tension, and improving the chance of its resuming its function. In cases fitted for it, the small aperture made by the canula might be enlarged by means of a tangle-tent, so as to serve the purpose of an artificial anus. It has been frequently found, after death, that a vast amount of the accumulation above the place of obstruction was sufficiently fluid to be removed in some such way as has been above hinted at.

In all cases of obstruction, a most restricted dietary must be observed, in fact, only enough given to support life. No solid or bulky food should be allowed; but small quantities of the most soluble and sustaining meat essences, milk, egg and brandy flip, ice, &c. The stomach must not be loaded even with water. Nutritive enemata will help much.

No reference need be made to exploded methods of administering mercury, shot, &c., to act mechanically in cases of obstruction; and such medicaments as tobacco-injections, strong coffee, ergot, nux vomica, &c. are now very seldom employed.

In volvulus and stricture, the chance of successful treatment is very small. By operation, the bowel may occasionally be reached above the place of closure. Internal hernias, even if recognised, are almost hopeless. For invagination low down large enemata, or the old Hippocratic plan of distending the bowel (now easily accomplished under chloroform) with air,

introduced by bellows or special instrument, from below, should be employed, if they can be carried out before the portions of bowel involved have become hopelessly fused together. Teliaferra's absurd idea of generating gas in the colon by the introduction of chemicals capable of evolving it, need hardly be mentioned. The inversion of the patient cannot do good; but the careful insertion of a bougie (possibly armed with a sponge) might prove advantageous in certain cases of intussusception pressing down near the anus. The strength of the patient should be well supported, and time gained for the occurrence of those changes in the bowel by which a spontaneous cure may be secured.

Stercoraceous accumulations must be mechanically removed. A lithotomy scoop or ordinary spoon may get at a good deal of the material; but a stream of warm water, made to play vigorously on the mass, or got more slowly to permeate and disintegrate it, by being allowed to come into contact with it through a long tube connected with a reservoir raised high above the bed is a better plan. A calomel and jalap purge will complete the cure. The long tube of O'Beirne is a great aid in many of these cases. The water current may be passed through it, and thus gain access to the bowel high up.

Colotomy and enterotomy, in cases of obstruction, have not hitherto yielded very promising results. In invagination, operation has been peculiarly unfortunate, though two recent cases, in which success was obtained, will probably lead to a further consideration of its merits. The difficulty of accurately diagnosing the condition of affairs, and the natural inclination to wait, in the hope of a favourable issue by the spontaneous elimination of the invaginated bowel, have hitherto opposed operation at a period when it might prove useful in intussusception. There is no doubt that, in a very considerable proportion of cases of invagination, a spontaneous re-establishment of the canal takes place; and also that, when gastrotomy (in itself most dangerous) was attempted, the condition of the bowel was found to be such that nothing could be done to relieve it.

In stricture of the lower portion of the bowel, colotomy should be performed; and that as early as the necessity is clearly present, and before the patient's strength is exhausted. More patients are lost by delay than die directly from the operation. The operation in the right loin (Amussat's operation) meets the requirements of some cases better than that in the left (Callisen's); while Nélaton's method of seeking for the cæcum, or at least the upper distended part of the bowel, in the right groin, has been much practised abroad, and with great success. Maissonneuve's suggestion, to excise a portion

of hopelessly obstructed gut, and unite the upper and lower portion by sewing them together, has never as yet succeeded; but an artificial anus might be secured after the portion of occluded bowel was found by gastrotomy, and it was seen that the closure could not be otherwise rectified.

Adhesions, bands, twists, loops, &c., are so difficult to recognise clearly, or to find even after the abdomen is opened, that gastrotomy (which, after other treatment has failed, is the only resource) offers but a slender chance of success. Inflammation becomes so soon developed as to make the operation very hopeless.

An exploratory operation should always be promptly performed, if obstruction occur and resist treatment, in the case of a patient who had been subject to rupture, and that even though there be no local evidence pointing to the seat of the former hernia as the place at fault.

It is in chronic cases that operative measures afford the most reasonable chance of success. If, after large enemata, the use of concentrated food, and a calomel purge at the right moment, the symptoms do not yield and the patient's vital powers begin to give way, the surgical art should be appealed to. It may be added, with reference to the use of enemata, that they require care and skill for their proper administration: and should when so much depends on their proper employment, be superintended by the medical man himself. If the fluid be not thrown up steadily and slowly, and in sufficient quantity, no good will be got; while if too much force be used, the bowel may be ruptured. The fluid should be retained as long as possible.—*British Medical Journal, Nov.* 25, 1876, *p.* 673.

38.—TAPEWORM EXPELLED BY TURPENTINE AFTER THE FAILING OF SEVERAL OTHER REMEDIES.

By Dr. Arthur Leared, Senior Physician to the Great Northern Hospital.

A middle-aged gentleman of stout make had been for some time under my care on account of dyspepsia with nervous depression. On the 6th of April last he passed about three yards of a tape-worm, of which he had not previously noticed any joints; he had been in the habit of eating beef and mutton very much underdone.

By my directions he fasted from noon, and took on the night of that day kamalæ ʒ ii. On the following morning, having fasted in the interval, he took another ʒ ii dose of kamala. The bowels were freely moved, and long portions of worm, including pieces of the neck, were expelled.

After this the patient looked very closely for indications of

the worm, and on June 20th, seven weeks and four days from the expulsion, observed a fully developed proglottis with the fæces.

The same precautions as to fasting were observed, and he took ext. filicis liq. ʒ i. (carefully made by Messrs. Dinneford from English rhizomes), followed in four hours by the same dose.

A mass of worm was expelled by the first dose, including some portions of the neck of such small dimensions that one hoped that the head itself would have been detached, or would have perished.

Nevertheless on September 15, twelve weeks and two days after the previous expulsion, proglottides were again observed. On September 30 the patient took with the same precautions as before, ext. filicis liq. ʒ iii., tincture kamalæ ʒ ii. in two doses, followed by castor-oil. Large portions of worm, with some narrow pieces, were again expelled. As a further precaution, after the interval of a week, the following draught was taken; succeeded, as in the other instances, by another of the same kind.

Ext. filicis liq. ʒ ii; pulv. kamalæ, ʒ i; mucil. acac. ℥ ss; infus, kusso. ℥ ii.

No portions of the worm were found in the evacuations, which owing to the action of kamala, were quite free. The only inconvenience experienced from the combination was nettle-rash and a sense of lassitude. The urine, as on other occasions, after taking male fern, was observed to be remarkably dark coloured; it was hoped that by following up the treatment by a combination of tænicides, each in full doses, the vitality of the worm would have at length succumbed. I had great confidence in kamala for tape-worm, having formerly used it largely with much success. But this hope was not realised. On December 12, seven weeks and three days from the last expulsion, portions of worm were again observed in the fæces.

I now directed the patient, after having fasted in the usual way, to take the following draught, and if it did not purge, a dose of castor-oil soon afterwards:—

Spir. terebinth. ʒ iii; t. hyoscy. ʒ ss; mucil. acac. ℥ ss; aquæ chl. ad. ℥ ii.

Free evacuations, containing the worm, together with the long-sought after head were soon caused by the draught alone. The hookless flat-shaped head at once proved the worm to be Tænia media cannellata. This worm is much more difficult of expulsion than Tænia solium.

My opinion, confirmed by this case, in which kamala and

extract of male fern were given in unusually large doses without destroying the worm, has long been that of all tænicides the old-fashioned one, oil of turpentine, is the most potent. But turpentine in large doses is not free from inconvenience, or even danger to life; I know of one death caused by its injurious action on the kidneys. This danger can be greatly diminished, if not altogether averted, by the precaution of giving a purgative, if the turpentine does not itself very speedily act upon the bowels.—*Practitioner, March* 1877, *p.* 170.

39.—ON SOME NEW TESTS FOR BILE PIGMENT.

By Dr. Walter G. Smith, Assistant Physician to the Adelaide Hospital, Dublin.

My object in writing the present paper is to invite attention to some tests for bile pigment, which, from their simplicity and freedom from fallacy, seem to answer admirably to the requirements of practice.

More than seven years ago a sentence in a short article by M. Maréchal, "Sur l'Urine," in the Journ. de Pharm. et de Chimie, Mars, 1869, attracted my notice. He there briefly states that tincture of iodine is a most sensitive test for bile in urine, giving with it a magnificent emerald green, which lasts for half an hour, and then passes through rose colour into yellow.

M. Maréchal's statements appear to have been overlooked or forgotten by subsequent writers, and I have not met with any allusion to his observations.

Since 1869 I have tried this test in almost every case of jaundice that came under my notice in the wards or dispensary of the Adelaide Hospital, and have repeated and varied the test many times in the one case. A few specimens of bilious urine, kindly furnished to me by other hospital physicians, have also been tested, *and in no single case have I failed to get the distinctive colour reaction.*

The best mode of procedure is to place about ʒ i. of the urine in a test-tube, and then to allow one or two drops of tinct. iodi (B.P.) to trickle down the side of the tube, held nearly horizontally, so that the two fluids may touch but not mix. If bile pigment be present, a fine green colour will almost immediately be developed below the red layer of iodine tincture. By holding the test-tube up against a white cloud, or against any white surface, in a good light, the three zones of colour will clearly appear—viz., the red iodine layer, and the yellow stratum of urine, separated by the green layer.

The test succeeds better by flotation in a test-tube than by

allowing the tincture of iodine to run into contact with the urine on a white plate.

If the colour of the urine be very dark, it is better to dilute it with at least an equal volume of water before applying the test. Should the quantity of bile pigment be very small, it is advisable to make use of a specimen of healthy, non-bilious urine as a standard of comparison, treating it similarly with the iodine solution. Very small quantities of the pigment can be detected by shaking up a little of the urine with one-fourth its bulk of chloroform. Allow it to stand, decant the supernatant urine, filter the yellow chloroform stratum, evaporate to dryness, and test. With the iodine test I have succeeded in showing the presence of bile pigment in the yellow sputum of one case of jaundice. A simple solution of iodine in spirit will not answer, on account of the precipitation of free iodine on dilution, and the liquor iodi and linimentum iodi are unnecessarily strong. Even the tinct. iodi (B.P.) may be sometimes advantageously diluted with spirit. It is important not to *mix* the iodine solution with the urine, and not to add too much of the iodine. From neglect of these precautions I have known the test fail in the hands of others.

The green colour usually lasts for a considerable time, and may persist for days. Gradually it turns to a dusky brown, but even then the green can sometimes be restored by the cautious addition of iodine. Heat changes the green speedily to brown, and previous boiling of the urine prevents the development of the colour. Nothing characteristic was observed when the green fluid was examined by the spectroscope. The *cause* of the coloration evidently lies in a limited oxidation, the bilirubin, as in Gmelin's test, being probably converted into biliverdin, with this difference, that other tints—red, blue, or yellow—are not evoked by the feebler oxidising power of the iodine.

With the object of examining the delicacy of the test, and of studying the effects of other oxidants, some further experiments were made; and my best thanks are due to Dr. Emerson Reynolds for the free use of the Laboratory, Trinity College, Dublin, as well as for several suggestions in the carrying out of experiments.

A sample of very dark-coloured bilious urine was diluted with four volumes of distilled water, and subjected to the following tests:—

Tincture of iodine brought out a bright green colour, while the same urine, similarly diluted, gave, with nitric acid, in Dr. Reynolds' hands, a pink colour, and but little or no tinge of green. The same urine, kept over until the day following, reacted similarly.

Freshly-made solution of bromine caused turbidity, and a scarcely perceptible greenish tint. Liquor chlori yielded a pale olive green. Five c. c. of urine from the same patient were diluted with five volumes of water, and afforded, with the tincture of iodine, a bright green; with nitric acid, a paler green, succeeded by zones of colour.

The same urine was submitted to the action of three other oxidising agents—viz., peroxide of hydrogen, chlorate of potassium, and nitrite of potassium.

To the diluted urine a little dilute sulphuric acid was added, and a few drops of commercial solution of peroxide of hydrogen. No colour was developed until a gentle heat was applied, when a pale green colour appeared, which was destroyed by boiling or by excess of the peroxide.

A few grains of potassic chlorate, dissolved in warm water and acidified with sulphuric acid, were added to the diluted urine. On heating the mixture a green colour was produced, similar to that caused by peroxide of hydrogen.

Nitrite of potassium and acetic acid added to the urine in the cold brought out a greenish yellow colour, destroyed by heat.

Dr. Reynolds then suggested a trial of another less known oxidising agent—viz., a solution prepared by digesting peroxide of lead (or, red lead) in glacial acetic acid, and allowing the sediment to subside. The solution probably contains an oxy-acetate of lead.

The following experiments were made with a small quantity of turbid, high-coloured urine, removed after death from the bladder of an old woman who died with ascites and intense jaundice due to complete occlusion of the bile-ducts. The urine was previously diluted with eight volumes of water.

(*a*) *Tincture of iodine* developed a permanent bright green; (*b*) *nitric acid*, green and red zones, the green colour rapidly vanishing; (*c*) *peroxide of hydrogen* and *acetic acid* gave a faint green in the cold; gently warming the fluid deepened the colour to a fine green, which was retained on cooling. (*d*) Some of the urine was added to a few drops of the *acetic solution of peroxide of lead;* a rich, clear green was immediately developed without the aid of heat, and remained persistent.

Solutions of lead peroxide in strong phosphoric acid (B. P. acid concentrated to one-fourth), and in glacial acetic acid *plus* five per cent. of sulphuric acid, were also prepared; and on another occasion some of the yellow peritoneal fluid from the same subject was tested with these solutions. The sulpho-acetic lead solution did not answer so well, but the phosphoric solution produced immediately a fine, permanent green colour, and rendered the turbid fluid perfectly clear, no doubt by dissolving

any albumenoid matters. Healthy urine gave no colour whatever with these solutions. *Ferric chloride* was also tried on the same fluid, and immediately developed a distinct green. The facility wherewith ferric salts are reduced is an obvious objection to this test.

All these results point to the conclusion that the green test is not a mere chromatic change, but is a true oxidation effect.

But it is not sufficient to show that iodine and various oxidants act as colour tests for bile; it is also necessary to demonstrate that no other pigment in urine, except bile, will give like results.

With this object I have taken every occasion of testing other specimens of high-coloured urines, non-bilious; and on this point especially—the crucial check of the value of the tests—I would be glad to learn the experience of others who have larger opportunities than myself.

Up to the present I have not been able to meet with a single source of fallacy in the use of the iodine test. Thus, in the deeply-coloured urines of continued fever, of acute pneumonia, of cirrhosis of the liver, of Addison's disease (for which latter opportunity I am indebted to Dr. Duffey), and in yellow vomit, I have uniformly obtained negative results. Bloody urine offers no difficulty, and the presence of albumen does not interfere with the test.

I have not yet had time or means to check the effects of the other tests described above, but, judging from analogy, there is no reason to anticipate any discrepancy.

Briefly to sum up the results of the foregoing observations, it would appear that the value and delicacy of the nitric acid test are less than is commonly supposed, although it is depended upon in delicate physiological investigations, and that it is desirable to have at command some supplementary tests for biliary pigment which shall be easy of execution and free from fallacy.

Four test-liquids seem to answer to these indications—viz., tincture of iodine, ferric chloride, peroxide of hydrogen, and the acetic or phosphoric solution of peroxide of lead. The two latter liquids possess over the former the advantage of being themselves colourless, whereby any change of colour produced in testing is more readily appreciated; and the phospho-plumbic solution especially promises well as a delicate reagent.

Peroxide of lead has been utilised in experiments on bile (Maly), but peroxide of hydrogen, and ferric chloride have not, to my knowledge, been used by other experimenters, and I venture to hope that physiologists and clinical observers will test their limits of usefulness.

With a fresh supply of material I purpose to study more

closely the action of the iron, lead, and hydrogen peroxide tests; but, as the clinical experiments have been chiefly conducted with the iodine test, I will, in conclusion, mention the reasons which seem to me to recommend tincture of iodine to the notice of those interested in such matters.

1. A single reagent, always easy to obtain, is alone necessary.
2. The test-liquid is not corrosive.
3. A single definite colour is produced with the bile pigment.
4. The colour is sufficiently persistent.
5. From the less powerful chemical energy of the reagent, as compared with that of nitric acid, there is a diminished liability to error.
6. No other pigment than bile will yield the characteristic green colour. And lastly,
7. The test fully equals in delicacy, possibly surpasses, the nitric acid test.—*Dublin Journal of Medical Science, December* 1876, *p.* 449.

DISEASES OF THE URINARY ORGANS.

40.—BRIGHT'S DISEASE AND ITS TREATMENT.

By the late Dr. Francis Sibson, F.R.S., Consulting Physician to St. Mary's Hospital.

We have evidence, amounting to a scientific demonstration, that in Bright's disease the poison in the blood causes arterial tension; and that the degree of the tension is in proportion to the amount of the poison. Although the arterial tension does immediate harm by causing hypertrophy and valvular disease of the heart and atheroma and dilatation of the aorta and the rest of the arterial tree, yet the mischief caused by the arterial tension is in every way secondary to that induced by the poisoning of the blood. Our treatment is, therefore, mainly directed to the poison, for we know that if we lessen that, we lessen all the ill effects that proceed from the poison. We have seen that the left ventricle, forced into a large expansion with thickening of its walls, has found relief by widening out its mitral aperture, and so sending its overcharge of blood backwards into the left auricle as well as forwards into the aorta. We have seen that the arch of the aorta, widened excessively by its own distension, has thus rendered its value incompetent, and has so lessened the tension of the whole arterial tree and lessened its own size. We have observed cases in which bronchitis or congestion of the lung has transferred the tension from the arteries of the body to the arteries of the lungs, which have in their turn relieved

themselves by pulmonary apoplexy, when the blood, incapable of finding its way through the capillaries of the lungs, has poured itself out into the air-cells. Under all these circumstances, and others that I do not name, the arterial tension has been completely relieved; and yet the poison saturates the blood and the tissues as completely as ever, sometimes more so, and the danger to life is often rather increased than lessened.

In considering the treatment of Bright's disease, I shall keep steadily in view the getting rid or lessening of the poison actually in the blood; the not adding to the blood fresh poison, either of the kind already there, or of a similar narcotic quality; and the stirring up the powers of life, so that they may better resist, override, and live above the poison that is there. I shall also keep in view the relief that may be afforded to certain effects of the disease, such as dropsy and distension of the abdomen, which tend dangerously to increase the disease that gave them birth.

Soft water, repeated frequently through the twenty-four hours, is the great and effectual diuretic. It is the needful medium for other diuretics if we would really make them do their work. Hard water certainly passes less freely through the kidneys than soft water, and in going through them it carries off much of the poison that has accumulated in the blood. I would here say that the kidneys cannot work by deputy. They alone have the power of expelling the urea and the other salts that are the result of the metamorphosis of the muscles and tissues. The hot-air bath, which is often an effectual remedy in acute Bright's disease and fatty kidney, draws off an abundance of water with common salt and salts that pass off through the surface, and it relieves the pressure of blood that is bearing on the kidneys themselves, and overwhelming or drowning their function. But this great remedy is doing the work not of the kidneys, but of the skin; and, as practical physicians, we all know how important it is that the skin should be active and warm in this disease. In doing so, as I have just said, the kidneys, no longer flooded with an excess of blood, are often restored to their function. I have advocated soft water, and the difficulty is to get it; for rain-water, if it could be met with, is unfit, and distilled water is unpleasant; and if much water is to be really taken, it must be welcomed by the palate. Schweppe's Malvern soda-water, the gas being previously stirred out with a spoon, is the best means that I know of giving soft water. It has the advantage, too, of lessening the acidity of the urine, for acid urine irritates the kidneys and makes them deny their function when affected with Bright's disease, especially in the inflammatory forms; just as

acid urine irritates the bladder when it, the prostate, or the urethra is affected.

This leads me to say that alkalies, in proper quantity, are as useful to the physician in Bright's disease, as to the surgeon in disease of the bladder and urethra; and for the same reason, since they render the acrid fluid bland and soothing that is constantly distilling through the bladder of the affected kidney. The smallest vessels then cease to shut themselves up, and the tubes are flushed by the abundance of soft mild fluid that is passed through them. We all know how admirably the potash salts often fulfil the purposes of which I have just spoken in these cases. Leeches to the loins, linseed and mustard poultices, warm and not wet, we all know to be effectual diuretics, as well as soothers, in acute Bright's disease.

When the abdomen is greatly distended by windy inflation of the stomach and bowels, or by ascites, or when the lower part of the body, from the waist downwards, is greatly swollen by dropsy, those conditions being either single, or, as they often are, conjoined, the return of venous blood from the kidneys is hindered by the general compression, and the urine is scanty. Under such circumstances, the relief of the swelling is often the best, and, indeed, only diuretic, for the kidneys refuse to let any quantity of fluid through themselves, when their veins are thus obstructed. The œdema of the surface should then be drained by puncture with the needle or lancet; and the ascites by the trocar and canula, or the aspirator.

One of my patients with Bright's disease in St. Mary's Hospital suffered from great distension of stomach and colon, moderate ascites, and some tightness of the skin of the abdomen from dropsy. He was only passing fourteen ounces of urine in the twenty-four hours. I decided to have the fluid removed from the abdomen; and, as the intestines came very near the surface, the trocar was thrust downwards and withdrawn. A drachm of fluid came through the canula, but, by the insertion of a female catheter, at hand for the purpose, through the canula, ninety ounces of fluid were easily drawn off. The relief to the patient was immediate, and the urine increased at once to forty ounces, and subsequently to sixty ounces in twenty-four hours. He improved for a time, but finally died. This case shows well how useless it is to give diuretics when the return of blood from the kidneys is hindered, for the same means exactly were employed before and after the tapping.

I have spoken here only of the obstruction to the flow of blood through the renal veins in such cases; but another effect is to have no vegetables, but take lemon-juice instead, and biscuits instead of bread. On this diet, the size of the abdomen has

lessened three inches in forty-eight hours, with great relief to the patient in every way.

It is evident that if we would put no additional poison into the body of the kind already there, we must know one of two things, either the character or the effects of the poison; so that we may put nothing into the body, whether in the form of food, stimulant, or medicine, that is of the same character or tends to produce the same effects as the poison already there.

My first lecture was occupied with the conditions that prove the presence of a poison in the blood, and in so far as that poison affects the circulation of the blood; and we saw the main effect of the poison on that function was that arterial tension, with all its various secondary effects, symptoms, and signs.

We have here a definite indication that we should avoid giving the patient anything that tends to increase arterial tension. We have the opposite effect to guide us in the treatment of fevers with great arterial relaxation, and its effect and sign, a dicrotous pulse; and we know that by giving those substances that increase arterial relaxation we do mischief, while by giving those that lessen arterial relaxation and substitute for it arterial tension, we do good.

A similar position holds in Bright's disease as regards the first point, and we may say with certainty that whatever increases arterial tension adds to the original mischief.

It does not, however, follow that whatever will lessen tension will be of use, for, as we have seen, the nitrite of amyl immediately converts the sphygmographic tracing of tension to one of complete relaxation or dicrotism. This step, however, instead of touching the disease, adds to the original poison another poison that intensifies rather than moderates the effects of the kidney disease.

It is self-evident that the main motive of our treatment, in regard to the point we are now considering, should be the withholding, as far as possible, every article of food that contains the same kind of poison as that which already contaminates the blood; and especially that we should not ourselves actually direct the patient to take such food as his proper nourishment. In carrying out this principle, it is needful that we shall keep steadily before us the character of the poison so infecting the body.

Although chemists and physiologists are agreed that the poison of Bright's disease consists in the *débris* of the tissues, that, instead of being expelled through the diseased kidneys, is unduly retained in the blood so as to impregnate every organ in the body; yet they have not yet discovered the precise chemical nature of that poison.

The work done by the muscles—including that most important

and incessantly labouring muscle of them all, the heart—and by every important organ, indeed, in the body, is done at the expense of some of the materials of those organs; for there can be no expenditure of force without the corresponding metamorphosis of tissue. No part of the tissue is lost, but it is converted mainly by oxidation into a series of new products, which are, at the beginning of the process of metamorphosis, only less complex than the muscle by the exuviæ of which they are formed; which become less and less complex in the descending scale; and which finally appear in the more and more simple forms of urea and ammonia. Now the juice of flesh, the composition of which has been in a great measure made known to us by the labours of Liebig and other chemists, is, if we deduct the albumen and gelatine, entirely composed of "extractive," due to the metamorphosis of tissue. Kreatin, one of the more compound of the structures transformed from muscle, is itself found in urine, and is converted by its decomposition into sarcosine and urea. The poison contained in the blood in cases of Bright's disease is composed of the materials metamorphosed from the muscles and other tissues, that accumulate in the blood because they are not sufficiently expelled by the diseased kidneys. Beef-tea and soups, deducting albumen, and Liebig's essence of meat, which contains no albumen, are entirely composed of the metamorphosed structures of muscle, including kreatin, kreatinin, and the fatty and aromatic acids. Some of those ingredients are innocuous, but the more important of them essentially correspond with the retained self-generative poison already circulating in the blood. By giving such patients beef-tea or the essence of meat or soups, we are adding the metamorphosed structures of the muscles of the cow to the retained metamorphosed structures that are already poisoning their blood. Beef-tea, and the so-called essence of meat, by containing the fragrant juices of the flesh, give a grateful spur to the stomach in patients weakened by fever and other ailments in which the blood is not poisoned by the effete tissues, and are of great value to such patients as the medium in which real food may be given. They are not in themselves a food, but flavour food that may be finely mixed with them, whether the *purée* powder of the meat of which they have been made, or vermicelli, or bread, or other farinaceous food. In these feeble patients the arteries are relaxed and the pulse dicrotous, and the metamorphosed materials from the cow's flesh increase the tension of the over-relaxed arteries. Those effete structures, though not a food, are a double spur when given to those weakened patients; first to the stomach, enabling it to digest real food, and then to the heart and blood-vessels, by lessening arterial relaxation or increasing arterial tension.

Cases of Bright's disease are diametrically opposite to the enfeebled patients of whom I have just spoken, in the character of the poisoned composition of their blood, the arterial tension caused by the difficulty with which their poisoned blood forces its way through the smallest vessels, and the powerful action of the heart. What is, therefore, a life-stimulating fluid to the exhausted patient, is an immediate poison to the patient with Bright's disease. Beef-tea, soups, gravies, chicken broth, animal fluids of all kinds, essences of meat, whether Brand's admirable kind, or Liebig's essence, which, because it is stale, should never be used where fresh beef-tea or essence is to be had, ought to be rigorously forbidden to every patient with Bright's disease.

It has been justly said that it is not incumbent on those who discover and expose an evil to find out and apply a remedy. The detective and critical mind is not, as a rule, the suggestive, organising, and artistic mind. The two classes of mind are almost opposed to each other. But in this case, in objecting to the use of beef-tea, essence of meat, and the like, with life to be supported by food, it is a duty that cannot be escaped from to find a substitute for the poisonous beef-tea; and that duty has already been practically fulfilled by every physician in this room; for in milk, abundantly supplied, we have a perfectly innocuous food that supplies every part of the body with its needed materials of repair; that has already built up and formed every one of us in infant-life; that promotes the action of the kidneys, and so tends to expel the poison from the blood; and that may be gratefully mixed with Schweppe's Malvern soda-water, or Apollinaris, or lithia, or other waters, with the effect of enhancing the purifying power of the kidneys and so washing out the poison. Milk, however, two or three or more pints daily, may be given in any form that is agreeable to the patients, cold, iced, hot, with rice, with bread, with flour or vermicelli, or corn-flour, with maccaroni, with fruit, with puddings and tempting well-flavoured dishes of all those varieties that the thoughtful, handy, artistic cook of taste and affection knows so well how to prepare.

The question must here be put and answered, whether it is proper or not to give meat to patients affected with Bright's disease. When we consider that meat contains its own juice which is impregnated with the poisonous metamorphosed structures; and that meat in its own nature tends to undergo metamorphosis; our first inclination would, perhaps, be to forbid meat. There is no doubt that meat is found to be injurious in some cases of acute Bright's disease, whether from cold or scarlet fever in the young, and one of yourselves told me after my last lecture of an interesting case of that class. In

answering this inquiry, however, we must look to both sides of the question, which every practical and sagacious physician will intuitively do, and inquire what influence the giving and the withholding of meat may have on a patient affected with Bright's disease. In cases of fatty, lardaceous, and granular kidney, meat in moderate quantities, and properly cooked, is in a practical point of view of unquestionable service. It gives them a compact non-distending food; it helps to sustain their muscular power and action, just as it helps to sustain hard labour, and it gives zest to life. Nor is this result of practice otherwise than confirmatory, as it always must be, of the physiological view of this question. Meat supplies the body with the exact ingredients of the patient's muscles, all ready to be laid down in the midst of the structure when it requires repair. It is like the stone already hewn and shaped in the quarry for the builder; transport only, and not local labour, is wanted. The meat that is eaten is not metamorphosed until it has served its purpose, taken part in the work of the muscle, and, like every other part so doing its work, is then metamorphosed and thrown off, first from the muscular fibre into the muscular juice, and thence into the blood and the urine. There is an experimental point suggestively in favour of giving meat in cases of Bright's disease that is quoted by Donders in his very valuable work on the Constituents of Food in relation to Muscular Work and Animal Heat, translated by Dr. Moore. He states that Bischoff and Voit observed that when a dog, after having been fed for some time on vegetable matters, got a liberal supply of meat, the water was removed from his body in streams; by the urine alone, four ounces more were excreted than was taken in with all the food and drink together.

I need not detain you by speaking of the valuable employment of eggs, of the fats, and of fat-making food of all kinds, the patient using his own choice; and of other non-poisonous and fitting articles of food, in cases of Bright's disease, for they suggest themselves to all of us in our practical everyday treatment of that affection.

The second application of the principle, that we should not add another poison to the poison in the blood, is to the alcoholic stimulants. The composition of alcohol, unlike that of the juice of meat, is quite different from that of the poison that infects the blood in Bright's disease, so that, in that respect, we have no reason to expect that alcohol would in those cases form an additional poison to that in the blood. There are, however, two of the effects of alcohol on the system—one from the immediate, the other from the habitual, use of that stimulant—that make it, when employed in large doses, an additional poison to that in the blood. The immediate effect is the intoxi-

cation of the nervous system; the effect of its habitual use is the degeneration of the tissues, and especially of the arteries, the actual production of Bright's disease itself. The full effect of the two poisons on the nervous system is very close, since they both produce confusion and clouding of the intellect, thickness of speech, imperfect use of the limbs, unconsciousness, and coma resulting in death. The excitement and exhilaration of alcohol, and the whole character of its early symptoms when not taken in excessive quantities, are quite unknown as the effects of the poison of Bright's disease. Looking to the analogous effect of the two poisions, when alcohol is given in large doses, and to the ultimate injurious effect of the continued over-use of alcohol in causing degeneration of the tissues and Bright's disease itself, we may accept the principle that, unless there be some immediate good effect to be gained from alcohol, it is better not to give it in Bright's disease, and especially not to give it habitually, or even in large quantities. Dr. Parkes has shown, by a long and careful series of observation of its effect on soldiers who were the models of health and strength, that the continuous daily use of alcohol in quantities above one ounce and a half, is injurious to the healthy man. It is self-evident that, if more than this quantity be injurious to the healthy man, less than this quantity will be injurious to the patient affected with Bright's disease which, as we have just seen, is itself often caused by habitual drinking, and which causes, like alcohol, degeneration of the arteries. We may say, then, with scientific and practical confidence, that in Bright's disease, alcohol, if habitually used, should be taken sparingly, and in far smaller quantities than in health.

There may, however, be some effect of alcohol in small doses that may render it of service in Bright's disease. In small quantities, largely diluted with soft water, and especially in certain forms, such as gin or whiskey among spirits, and hock and perhaps claret among wines, it certainly promotes the action of the kidneys, and in so doing it may carry away not only its own poison but a certain amount of the poison of Bright's disease.

Thus controlled, alcohol may be of use in that disease, for we know that practically it is sometimes of the greatest use. Hock was a favourite wine with the original observer of this disease, Dr. Blackall of Exeter; that wine or claret in very moderate quantity—a glass or at most two daily of either of those wines largely diluted—may be, and I believe is, of real service in some cases. It promotes the action of the kidneys, serves to help digestion, and to give a sense of support and comfort. Whiskey in very moderate quantity, an ounce in the twenty-four hours, very largely diluted, may prove equally serviceable

with hock or claret in its action on the kidneys. If, however, there be any cirrhosis or congestion of the liver, or any degeneration of the arteries, spirit is to be altogether forbidden, and every wine that contains spirit. Beer is as a rule to be forbidden, but I know no practical objection to a moderate amount of cider.

The effect of alcohol on arterial tension is an important consideration in guiding us as to its use in Bright's disease. Dr. Anstie showed, in 1868, that alcohol increased arterial tension and the power of the heart, and the force of its action in cases of fever or pneumonia with low arterial tension, indicated by a dicrotous pulse; and he thus supplied us with a scientific and practical rule of the greatest value for its employment in those cases. If alcohol increased arterial tension in health, it would at once condemn its employment in Bright's disease, with arterial tension, which, as we have already amply seen, is the natural effect of that disease.—*British Medical Journal, Feb.* 10, 1877, *p.* 155.

SURGERY.

AMPUTATIONS, FRACTURES, DISLOCATIONS, AND DISEASES OF BONES, JOINTS, ETC.

41.—NEW METHOD OF AMPUTATING AT THE HIP-JOINT.

By David Newman, Esq., House Surgeon, Western Infirmary, Glasgow.

Amputation at the hip-joint, though necessarily a dreadful operation, has become much more frequent since the introduction of chloroform, which, doubtless, deprives it of much of its horror, as well as diminishes the immediate danger of such an extensive wound. But however much chloroform may assist in making the operation less disagreeable to the operator, and, to some extent, safer to the patient, yet it is of but little avail in arresting the flow of blood, the loss of which the patient is ill able to afford. It therefore devolves upon the surgeon to do all in his power to prevent the loss of a fluid which is so precious, and the saving of which may turn the tide in favour of the patient. This he is able to do in all cases of operation upon the extremities, when the seat of the operation is sufficiently low down to allow of the application of pressure, the method advocated by Prof. F. Esmarch being probably the most effective. But, however easy it may be to apply pressure to a limb in its continuity, it is entirely different when, in order to check the hemorrhage, the pressure requires to be applied to the artery as it passes over some part of the trunk,—to the aorta, or to the femoral as it passes over the pubis, in cases of hip-joint amputation, and the subclavian in that of the shoulder. It has been the custom in these cases to amputate by transfixion, and as the knife advances it is followed by the fingers of an assistant, which are introduced into the wound behind the knife, so as to command the arteries previous to their division, whilst another assistant is ready, as soon as the first flap is cut, to compress the vessels in the other flap, either by a sponge or with the fingers. But, even with these precautions, together with pressure upon the femoral as it passes over the brim of the pelvis, or compression of the aorta, as recommended by Professors Lister and Pancoast, the operation requires to be performed with rapidity, in order that dangerous

hemorrhage may be prevented. The arteries, as they are uncovered one by one, by the assistants in order to be secured, pour forth part of their contents, which in each individual vessel may not be very large, but, when taken collectively, the amount of blood lost is considerable, and well worth trying to preserve. No doubt the use of the aortic tourniquet, if properly employed, will, to some extent, decrease the flow of blood through the vessels below the point of application, but the difficulty of applying this instrument, and of keeping it in proper position, is so great, that its adoption is by no means general, besides the danger af applying it too forcibly, or in such a manner as to impede the venous return through the vena cava, are points not altogether unworthy of consideration. Lister, instead of using the aortic tourniquet, adopts Esmarch's elastic band, wound tightly round the waist, three or four times, a towel having been interposed between it and the skin, and a pad placed over the aorta on a level with the bodies of the lower lumbar vertebræ. Esmarch also employs a similar method. When we consider the unavoidable dangers of anæsthetics, of which we have only too frequent evidence, as shown by the number of deaths during their administration, it is the duty of the surgeon to be extremely careful not to interpose any agency, which might increase the risk of interference with the circulation or respiration. It cannot, I think, but be admitted that the application of a tourniquet to the aorta, which may also compress the vena cava, particularly Esmarch's elastic band round the waist, must interfere, to a great extent, both with the circulation and respiration. The employment of Esmarch's band, must, by its pressure upon the abdominal muscles, and the abdominal contents, which, in their turn, press upon the diaphragm, interfere with respiration, because in this way two sets of muscles, inspiratory and expiratory, are, as it were, to a certain extent, withdrawn, and no longer lend their aid in carrying on a function so necessary to life; besides, the injurious effects which might result from such powerful and continuous pressure upon the sympathetic ganglia and nerves, and upon the viscera. In the method which I propose, the pressure is neither applied to the aorta, nor to the femoral as it passes over the brim of the pelvis, but is brought to bear upon the base of the flaps. In order to accomplish this, however, it is necessary that certain modifications, both in the instrument and the mode of operating, should be adopted. I shall first describe the appliances, and then go on to consider the necessary alterations in the operation.

The instrument may be described as follows:—It consists of a knife, the blade of which is twelve and a-half inches long, and one inch broad. Measuring from the handle, the first nine

inches is half-an-inch broad, and the remaining three-and-a-half inches, an inch broad, and tapering to a point. Into the space thus left in the back of the blade, two steel slips are fitted, nine inches long, two-fifths of an inch broad, and one-tenth of an inch thick. At the point end of each slip, a small oblong hole is made, with a pin passing across it. To the handle end of each slip, an elastic band is attached, in such a way as to allow of the band being elongated or shortened as the case may be. To the end of the elastic band, a hook is attached, which is used to connect the elastic band with the other end of the steel slip. The back is stretched from the handle to the broader portion of the blade. The parts, when put together, are perfectly flush, and in no way interfere with the knife when used in cutting. The handle is four-and-a-half inches long, and has two small snibs, which are used to keep the steel slips in position.

[These details will be best understood by referring to the lithographic plates in the paper in the Glasgow Medical Journal.]

The method of performing the operation most conveniently is by making antero-posterior flaps. Before proceeding with the operation the limb should be tightly bandaged either with a wet calico, or an elastic bandage, in order to press out all the blood from the vessels of the limb; and when this is inapplicable, the limb should be elevated and pressure applied by the hand. After this has been done the patient should be put under the unfluence of an anæsthetic, and the breech drawn down to the margin of the table, and secured there by a bandage passed round the sound limb and across the pelvis, and made fast to the leg of the operating table. The diseased limb, which, for the sake of description, we will suppose to be the right, should be confided to the care of a trustworthy assistant, whose duty it is to alter the position of the limb as required during the different stages of the operation.

The thigh should now be abducted and slightly extended, so as to relax the tissues posterior to the bone. The surgeon stands on the left side of the limb, and, holding the knife, inserts the point a little in front of the tuberosity of the ischium, and passing it outwards posterior to the neck of the femur, brings it out an inch behind the line between the anterior superior spinous process and the trochanter major. In passing the knife, as soon as the point is found to have come in contact with the neck of the bone, the limb should be rotated inwards, so as to allow the point of the knife to pass out as close to the line between the anterior superior spinous process and the great trochanter as possible. The knife is now lying posterior to the neck of the femur, and almost parallel to Poupart's ligament.

The posterior slip should then be removed from the side of the knife. The knife is now withdrawn, so far as to allow the point to be passed anterior to the neck of the femur. The limb is abducted and rotated outwards, so as to allow the point of the knife to emerge from the same wound as it did in introducing the posterior slip. The next part of the operation is to extend the elastic band and hook it, in order to compress the posterior flap, which having been done, the vessels of this flap are so compressed between the elastic band and the steel slip that bleeding from them is impossible. The elastic band of the anterior slip is now stretched and fastened, and the slip is disengaged from the side of the knife. The position of matters is now as follows :—The posterior slip is compressing the posterior flap, the anterior slip compressing the anterior flap, and the knife lying in front of the neck of the bone close to the anterior slip. The knife is now carried downwards, and made to perform a sawing movement, so as to form the anterior flap. The thigh, which, during this stage of the operation, has been flexed, is now extended, abducted, and rotated outwards, so as to allow the surgeon to open the joint by cutting upon the head of the bone in front. As soon as this has been done the femur is depressed, causing the head of the bone to start forwards ; the round ligament and the remainder of the capsule are now cut, the knife brought round the head of the bone, and the posterior flap is formed by cutting between the posterior slip and the bone, and then downwards and backwards through the tissues at the back of the thigh.

The amputation having been completed, it will be seen that both the anterior and posterior flaps are compressed between the slips and the elastic bands, so that the escape of blood from the vessels of either flap is impossible. The surgeon should now direct his attention to the femoral artery, and the vessels of the anterior flap, that require to be secured, after which the band may be removed and the vessels of the posterior flap tied in turn, and the slip and band compressing it may be detached.

I have described the method of operating on the right side, after which it is quite unnecessary to enter into a detailed description of the operation when the left limb has to be removed, the only difference being that the knife is entered between the anterior superior spinous process and trochanter major, and brought out in front of the tuberosity of the ischium, instead of *vice versa*, as it is on the right side. The way in which the limb is moved during the different stages of the operation is also different, as will be evident ; the object, however, being, in the first instance, to bring the point of the knife out by the same wound, both in passing the anterior and posterior slip, and

after the anterior flap has been cut, to disarticulate the head of the bone.

The advantages which are associated with the above method of amputating at the hip-joint, appear to me to be numerous. One of the first, a not inconsiderable one, is that the operation may be performed without the fear of serious hemorrhage, and without interfering with the respiration or circulation during the administration of anæsthetics, no pressure being applied round the waist, or to the aorta.

The bands and slips so grasp the flaps, that bleeding from the vessels is very slight indeed. I tested this on the dead subject, by having fluid injected into the common iliac artery whilst I was removing the limb. The force used in injecting the fluid, was considerably greater than that exerted by the blood column during life, and the result was very satisfactory, the amount of pressure required to obliterate the artery being much less than I imagined.

Another advantage is, that the surgeon may take his time in operating. The object of performing the operation rapidly, viz., the fear of serious hemorrhage, being removed, he does not require to hurry over the operation in order to secure the vessels. As long as the bands and slips are in position he may have perfect confidence that the amount of blood lost will be but trifling.

The assistants required to assist the surgeon are fewer than those required to perform the ordinary operation. Thus, the number usually required is as follows:—(1) One to administer chloroform; (2) another to keep the aortic tourniquet, when it is used, in position; (3) another assistant is required to compress the femoral; (4) to hold and rotate the limb, as required, during the different stages of the operation; (5) one to hold the anterior, and (6) another the posterior flap when cut; (7) to pass the instruments, sponges, &c., to the surgeon. This may be all very well when proper assistance can be procured, as in hospital or city practice; but it is otherwise when a country surgeon is called upon to perform the operation with one or two assistants, on whom he cannot always rely. Besides, even when a proper number of good assistants can be had, they are of necessity standing so close to one another, that the movements of the surgeon are much embarrassed.

When the mode of operating, above described, is adopted, the assistance required is much less. Those necessary are (1), one to administer the anæsthetic; (2) one to hold and rotate the limb; (3) another to hold the anterior flap when cut, the posterior being allowed to hang down, compressed at its base by the steel slip and elastic band; (4) an assistant to hand the

instruments, &c., to the operator.—*Glasgow Medical Journal, October* 1876, *p.* 449.

42.—EIGHT CASES OF EXCISION OF THE KNEE-JOINT, TOGETHER WITH A DESCRIPTION OF A NEW MODE OF PERFORMING THE OPERATION.

By William Knight Treves, Esq., Surgeon to the National Hospital for Scrofula, Margate.

Excision of the knee-joint has got into bad odour of late. I believe the explanation is to be found in the fact that operating-surgeons either do not always sufficiently secure perfect immobility of the limb in the putting up, or leave the after-dressing to dressers or house-surgeons of limited experience.

It has been our custom not to remove the limb from the splint in which it was placed on the operating-table, till the wound is healed and union is believed to have taken place; measures being taken to prevent the soiling of the splint, so that removal may not be necessary. The splint has generally been left on for three months. In all these cases, the limb has been secured to the splint by gum and chalk or silicate bandage, so as to render rebandaging unnecessary. The splint used has consisted of moulded leather thigh and leg pieces, with iron interruption at the knee; the leather splint being put on damp, and secured by the fixed bandage at the time of operation. Sufficient slices have been taken off the bones to permit the excised ends to lie easily in a good position, and not to exercise any pressure against each other. All the cases have had an excision-bed, that is, two short mattresses separated by a square air-pillow, which is emptied for the use of the bed-pan, or to change the sheets. The limb has in each case been swung in a Salter's cradle. They have all, except the fatal case and one still in the hospital, been kept in till the wound has entirely healed, and they have been able to walk, run, and use the limb freely. Their average stay has been about six months after the operation. I have mentioned these particulars at the commencement to avoid repetition.

1. R. M., aged 17, female: disease of two-and-a-half years' standing; acute pain on movement, with partial dislocation outwards; excised March 17th, 1871—usual operation. The cartilages were found eroded, and there was grumous matter in the joint. This patient has remained perfectly well up to the present time. She walks long distances and leads an active life.

2. E. A., aged 11, male: disease of seven years' standing; complete dislocation of tibia backwards and perfectly useless limb; excised October 12th, 1871. In this case, I for the first

time tried the side-incisions; but, difficulties arising from the malposition of the bones, I completed the incision and performed the ordinary operation. The result was good; he could run and play leap-frog before leaving the hospital, and remained quite well a year afterwards.

3. H. B., aged 11, male; disease of two-and-a-half years' standing; operation August 29th, 1872; side-incisions and patella left. There were ulceration of cartilages and total disorganisation of the joint. The result was good, with a moveable joint.

4. M. B., aged 23, female; disease of eight years' duration; operation October 19th, 1872; side-incisions; patella sliced. There were gelatinous degeneration of the synovial membranes and ulcerated cartilages. The result was good. She was leading an active life up to last Christmas.

5. G. W., aged 8, male. His knee had been excised at a London hospital, and he came down with the tibia completely dislocated backwards, the leg hanging like a flail to the thigh, without any bony union. I excised again, with a perfectly good result, as shown in the photograph. (Photographs were shown of all these cases, except the fatal one and that last excised.) This dislocation, I found reason to believe, might have been brought about by the muscular throes attending masturbation, to prevent which vice I removed the foreskin.

6. M. S., aged 20, female: operation August 20th, 1874, by side-incisions; gelatinous degeneration of the synovial membrane, with erosion of cartilage. This was a most promising case; but the patient died a fortnight afterwards from pyæmia, and there was reason to fear that she had been infected by a sponge which had been used in a case of septic poisoning. I have never used a sponge since.

7. E. H., aged 8, female: disease of three years' duration; excised September 22nd, 1875, by side-incisions. The bone-surfaces were carious, with numerous sinuses about the joint. Some of the sinuses were difficult to heal, and this patient is only now leaving the hospital. She has a perfectly sound limb.

8. W. S., aged 9, male; excised July 18th, 1876, by the ordinary operation. I did not use the side-cuts in this case, as the patella was much diseased, and there were sinuses and wounds about the front of the joint. The ends of the bones were found carious. The wound has healed, and he is now getting about on crutches, wearing the splint applied at the time of operation.

I will now, with your permission, describe a new mode of performing the operation, which I may fairly call my own, and

which, I venture to think, has in suitable cases certain decided advantages over the usual plan. The object of this operation is to leave the tissues in front of the joint uninjured, to preserve the natural covering of the joint, and to keep intact the extensor tendon with its attachments. The bones are sawn *in situ*.

First. I make a semilunar incision about three inches in length on each side of the joint, the lowest point of each incision being thoroughly dependent for the exit of pus or serum.

Secondly. I divide the lateral ligaments on each side, and reflect the tissues till the synovial cavity in front is well opened. If there be adhesions in front, they are divided. A wide director is passed behind the joint in front of the posterior ligament, and with a narrow bistoury the crucial ligaments and any adhesions there may be between the bones are divided.

Thirdly. A metal retractor is inserted in front of the bones to secure from injury the tissues in front and the skin and tissues loosened from the sides, whilst the bones are being sawn. The blade of a Butcher's saw is passed behind the joint; and, this being connected with its frame, a thin slice is sawn from the joint-ends of each bone. The sawn surface of this slice is the exact counterpart of the surface left behind; and if, on examination, it appear to be healthy, I pass on to the patella, which is left if healthy, or sliced if its cartilage be ulcerated. I formerly used a chain-saw for the sections, but have relinquished it in favour of Butcher's.

The following are the chief advantages to be derived from this mode of operating.

1. Decided improvement in the after-appearance of the limb. The front view shows little difference from the other limb.

2. Greatly increased power of extension. After ordinary excision, extension is often feeble from the divided and shortened extensor tendon; the leg is inclined to drag, and the patient catches his toe in walking. With this operation, they are able to lift the leg even before union is firm; and they get increased advantage from the additional power and handiness of the limb.

3. The extensor tendon being still attached to the tibia in front, whilst the posterior ligament is intact behind, the bones are not so loose and the tibia is not so likely to become displaced.

4. This mode of operating partakes of the nature of a subcutaneous operation. The sawn surfaces are still left under their natural covering; they are not exposed under an extensive wound, which will sometimes gape in spite of care; but, being well protected, they unite, I believe, more kindly and readily than with the usual operation. I may add that this is, after a little practice, a very easy operation.—*British Medical Journal, Feb.* 3, 1877, *p.* 133.

43.—SOME REMARKS ON EXCISION OF THE ELBOW-JOINT; WITH TWO CASES.

By Surgeon-Major J. H. Porter, Assistant-Professor of Military Surgery, Army Medical School, Netley.

There is probably no operation in surgery in which more satisfactory results may be obtained for the patient and surgeon than that of excision of the elbow-joint, provided the cases are judiciously selected, the operation properly performed, and the after-treatment carefully carried out; and yet examples are frequently met with in which, from want of due precautions, the patients are encumbered with useless limbs.*

Recently two patients came under my observation in whom the elbow had been excised with most unsatisfactory results; in one he had lost all motion in the shoulder, elbow, and wrist-joints, as well as that of the hand and fingers of the affected limb, due to want of attention in the after-treatment; and in the other there was no power of extending the forearm, and the patient was unable to flex the fingers, which had become stiff and straight. In this case the want of success was attributable to the method of operation, which was by the H incision, and inattention to the after-treatment.

Such unfortunate results naturally induce the surgeon to guard against and prevent similar consequences in his own practice; and in investigating the experience of others, I was much impressed with the importance of the observations made by Mr. R. Hodges, of America, and Mr. C. T. Maunder, of London, respecting the method of operation to secure extension of the forearm. The former remarks that, in excision of the elbow, no transverse cut across the triceps should be made, and the latter (*vide* British Medical Journal of July, 1871) that it is essential to securing extension of the forearm, which power is not unfrequently lost, and for which he says the operation is responsible to preserve those tendinous fibres of the triceps muscle which are sent from beyond the attachment to the olecranon to blend with the fascia of the forearm, and especially with that portion of the fascia overlaying the anconeus muscle. Mr. Maunder commences the operation by a longitudinal incision at the back of the limb, in length three or four fingers' breadth both above and below, and crossing the point of the olecranon. He next sinks the knife deep into the triceps muscle, and divides it also longitudinally into two portions, the inner one of which is the more firmly attached to the ulna, while the outer portion is continuous with the anconeus muscle,

*I therefore feel it is unnecessary to apologise for publishing two cases, in which I trust, some of the circumstances connected with them will be found interesting, if not instructive.

and sends some tendinous fibres to blend with the fascia of the forearm. It is these latter fibres that are to be scrupulously preserved.

To these suggestions of Drs. Hodges and Maunder I paid strict attention in the two cases to be hereafter detailed, and with the happy result of obtaining most perfect power of extension. It is scarcely necessary to observe that, with ankylosis in the straight position, there may also be power of extension; but as that result is not the only one desirable, it is necessary to try and induce the power of flexion so as to produce a generally useful limb, such as one possessing the natural motions of the shoulder, wrist, forearm, hand, and fingers. To obtain these results but little has been said by authorities, except as regards the movements of the elbow, and on these points opinions differ as to the period at which motion should commence. This, of course, might be influenced by the condition of the patient and the state of the wound.

In the two following cases the limbs were first simply laid in an extended position on a firm pillow, and the wound dressed with lint saturated with carbolised oil, and cold applications to the outside. In both cases slight movement of the fingers was commenced the day after operation; about the fifth day supination and pronation of the forearm, and as soon as the inflammation consequent on the operation had subsided, flexion of the elbow, with slight extension, to prevent the ends of the bones coming into contact.

When the patients were strong enough to sit up and go about, exercises were enforced with a weight suspended to a cord passed over a pulley, which brought into action all the muscles and joints of the affected limb.

The electric induction current was also used with decided advantage.

Case 1.—T. W., a delicate-looking lad, fifteen years of age, belonging to the Duke of York's school.

Family history of phthisis, his father and an uncle having died of that disease.

In March, 1875, he had a sharp attack of rheumatism, which affected the elbow and knee-joints, confining him to bed for two months, at the end of which period the swelling of the joints had subsided, with the exception of the left elbow, which remained swollen and painful.

In August he was received into Netley Hospital for change of climate, his general health being impaired. The left elbow was uniformly enlarged, but there was no bulging of the synovial sac. There was slight pain in the head of the radius, increased by rotating the hand. The forearm was slightly flexed, and the elbow one inch larger in circumference than the right.

His general health improved, but in October an opening of a sinus appeared on the inner aspect of the arm, about one and a half inches above the condyle, through which a probe could detect exposed bone; there was, however, no decided evidence as to the exposed bone being connected with the joint, and he was treated with tonics and nourishing diet, the joint being kept at rest, with a hope that the diseased bone might be thrown off.

There was very little change till the beginning of December, when the elbow became much enlarged, and as compared with the sound one, was three inches larger in circumference. The skin over it had become pale and mottled, and the cutaneous veins enlarged, but there was no great amount of pain.

On the 3rd of December he accidentally struck his elbow against a table, which set up active inflammation, and early in January, 1876, when all inflammatory symptoms had subsided, it became apparent that the joint was so extensively diseased that excision would be necessary.

On the 10th of January I excised the joint by the longitudinal incision, the limb being rendered bloodless with Esmarch's bandage, and the patient being under the influence of ether.

After the operation the limb was placed in a straight position on a firm pillow, no vessels were tied, and there was very little hemorrhage.

On the 11th the lad was directed to flex his fingers, which he did with some difficulty, but without pain. On the 15th of January, or the fifth day after the operation, he flexed his fingers with ease, which were perfect in sensibility, and slight passive exercise of the forearm was commenced, so as to secure pronation and supination, as well as flexion of the elbow-joint.

These exercises were practised daily till the 8th of February, when he commenced the use of a 1 lb. weight suspended by a cord over a pulley, which he worked by a little help from the sound arm.

His general health gradually improved, and on the 1st of March the wound of operation had quite healed, and the arm was daily becoming stronger.

On the 3rd of June he was discharged from hospital, it being a few days less than five months since the operation. He could then perform the following motions—scrub and sweep the floor, lift a considerable weight, place his hand behind his back so as to button his trousers, supinate or pronate the hand, extend the forearm, brush his hair, and feed himself with a fork. The motions of the shoulder-joint were perfect, as well as that of the wrist, hand, and fingers. The sensation in the limb was similar to that in the sound one, and he possessed in every respect a useful arm, which would enable him to earn a livelihood.

Case 2.—Sapper G. S., Royal Engineers, twenty-eight years of age, of healthy appearance, with a good family history. In 1868 he struck his left elbow when mining, after which it became swollen and painful. This passed off, but in 1870, when quartered in Bermuda, it again became affected, but after suitable treatment the disease disappeared. In 1875, when quartered at Gibraltar, he again hurt it, since which time he has been unable to use it, the joint having become swollen, and painful on pressure. He was invalided to England, and arrived at Netley Hospital last December, when the joint was found much swollen and painful, but no symptoms to denote disease of the cartilages or bone.

In April, 1876, suppuration set in, and several abscesses formed, which were opened in front of the joint. His health now became much impaired, and it was found on examination that the joint was implicated.

On the 2nd of May I excised the joint by the longitudinal incison, the limb being rendered bloodless with Esmarch's bandage. The operation was followed by considerable oozing, necessitating the opening of the wound. The day after the operation movement of the fingers was commenced. On the 7th of May, or fifth day after the operation, pronation and supination were commenced, and on the 12th flexion and extension of the forearm. On the 31st of May he used a 1 lb. weight suspended over a pulley, which was increased up to 4 lbs. on June 15th.

By the 14th of July the wound of operation had completely healed, and, feeling strong and well, he attempted mowing, and cut down one hundred yards of grass; which he says he performed with little inconvenience.

With the exception of the power of flexion of the forearm, all the motions of the limb are perfect, but in this he is daily improving, being able to feed himself with a fork, and brush his hair; he can also button his trousers, place his hand behind his back, and lift considerable weights.

The muscles of the arm are fairly developed, as may be seen by illustration No. 2.

In Case No. 1. there was gelatinous disease of the joint with caries of all the bones.

In Case No. 2. there was pulpy disease of the joint and extensive caries of the humerus and ulna.

The amount of bone removed in both cases was about two inches.—*Dublin Journal of Medical Science, Oct.* 1876, *p.* 281.

44.—ON EXCISION OF THE JOINT BETWEEN THE OS CALCIS AND ASTRAGALUS.

By Thomas Annandale, Esq., F.R.S.E., Surgeon to the Edinburgh Royal Infirmary, and Lecturer on Clinical Surgery.

Excisions of certain of the bones and joints of the foot for the removal of disease are now well-recognised operations.

In this paper I intend to confine my remarks to the conservative surgery of one joint of the foot, namely, that between the os calcis and astragalus.

Although the existence of disease originating in and confined to this joint has been long recognised by surgeons, no attempt has, so far as I am aware, ever been made to act upon the diseased joint alone, and remove by excision its affected surfaces. Some authors advise that, in disease of this joint, either the entire os calcis or astragalus be excised, and the remaining diseased surface gouged away. Others, as the late Mr. Syme, advise, and have largely practised, amputation at the ancle for this disease.

The apparent difficulty of obtaining free access to this joint, and of removing its surfaces, is, no doubt, the reason which has hitherto prevented surgeons performing re-section of it; but having recently successfully practised a thoroughly efficient method of excising this articulation, I venture to hope that, in describing this method, I am doing some little to promote the progress of practical surgery.

The importance of recognising and treating early disease originating in this articulation, especially after suppuration has taken place, must be acknowledged, for, owing to the situation and connexions of this joint, there must always be a peculiar risk of the gradual implication of the surrounding bones and joints.

Having carefully studied the anatomical relations of the joint under consideration, I found that its entire extent could be best exposed without injury to surrounding structures in the following way: The foot having been placed in the extended position, and resting on its inner aspect, an incision, commencing about an inch above the tip of the external malleolus, and carried along its posterior border in a curved direction to the calcaneo-cuboid joint, thoroughly exposes the posterior and external portion of the joint, when the peroneal tendons have been drawn outwards, and some ligamentous bands divided. This incision will be found to run along the inner border of the tendon of the peroneus brevis muscle. The anterior and internal portion of the joint can then be exposed by placing the foot still in the extended position on its outer aspect, and

making an incision from the tip of the internal malleolus along the course of the tendon of the tibialis posticus, as far as the prominence of the scaphoid bone, drawing forwards this tendon, and carefully drawing backwards the other tendons and the posterior tibial vessels and nerve. By making the first incisions through the skin and cellular tissue only, and so ascertaining the exact position of the tendons likely to be injured, then cutting down through the periosteum to the bone, and with a periosteal scraper separating to a sufficient extent the periosteum, together with all the other superficial tissues, there is little risk of injuring any of the tendons or other important structures.

Both aspects of the joint having in this way been exposed, it will be found that by means of the chisel and mallet the articular surfaces can be easily and accurately removed, the posterior portion being removed through the external incision, and the anterior portion through the internal one. Should there be any disease in the hollow or fossa between the two articular surfaces, it can be readily reached and removed with the chisel or gouge through either of the incisions.

I have now to relate the following case, which is not only a proof of the possibility and success of this operation, but is also an illustrative example of the form of disease in which I think that excision may, instead of amputation or removal of an entire os calcis or astragalus, be employed.

Catherine K., age 18, was admitted into my wards in the Infirmary on the 20th of March of this year. Her history was, that about two years previous to her admission she severely twisted her right foot. From that time she suffered more or less pain in the region of the joint between the os calcis and astragalus, with swelling of the foot below the ankle. She was treated by rest and the application of warm fomentations, tincture of iodine, and other remedies, but with only very temporary benefit. As her symptoms were becoming steadily worse, she requested to be admitted into the infirmary.

On admission the symptoms of disease in the calcaneo-astragaloid articulation were well marked. These symptoms were swelling on the lateral and posterior aspects of the foot immediately below the malleoli, tenderness on pressure, and pain when the foot was firmly grasped, and moved laterally and rotated. The ankle-joint was healthy, and the anterior tarsus and portion of the foot were free of disease. The first treatment consisted of complete rest, and the application of soothing means to relieve the pain. Shortly after this a small abscess formed on the external aspect of the joint, and it was opened and the wound dressed with antiseptic precautions. The wound was healed in about a month, and the foot, ankle, and leg were

then confined in pasteboard splints and a starch bandage. With this appliance she was sent to the Convalescent Hospital for six weeks, but on her return little change had taken place in the symptoms. The splints were reapplied, and she went home, but returned again on the 20th of July, as the disease was no better, but rather worse. The swelling and pain were now more marked, and in addition there was a feeling of deep fluctuation over the outer aspect of the joint. On account of this aggravation of the disease she was readmitted, and on the 29th of August I exposed the lateral aspects of the joint in the way I have described, and finding suppuration with caries of its articular surfaces, more particularly of those of its posterior portion, I removed with the chisel the entire articular surfaces of the joint—that is to say, the articular surfaces on the upper aspect of the os calcis, and the corresponding surfaces on the under aspect of the astragalus. The interosseous ligament was softened and in part destroyed. After the diseased surfaces had been removed, a drainage-tube was passed completely through the joint, and its open ends allowed to project at either wound. The operation was performed with antiseptic precautions, and the usual antiseptic dressing was afterwards applied and continued until the 10th of October, when the boracic-acid lotion was substituted for it. The patient's progress after the operation was excellent, and the wounds gradually contracted, and were soundly healed on the 22nd of November.

At this date the appearance of the foot was natural, and the movements at the ankle-joint were free and perfect. The patient could bear good weight upon the foot, and it promised to be in time quite strong and useful.—*Edinburgh Medical Journal, Jan.* 1877, *p.* 584.

45.—THE TREATMENT OF COMPOUND FRACTURE BY CLOSURE OF THE WOUND WITH COMPRESSES SOAKED WITH COMPOUND TINCTURE OF BENZOIN.

By Thomas Bryant, Esq., Surgeon to Guy's Hospital.

[Mr. Bryant gives an interesting series of fourteen consecutive cases of compound fracture treated by him by closing the wound as soon as possible after the accident with lint saturated with compound tincture of benzoin. The favourable opinion which, from frequent inspections, we had formed of this clean and simple mode of practice led us to examine the records of a large number of cases. A perusal of the subjoined notes shows that the results obtained are almost uniformly satisfactory.]

Case 1.—*Compound comminuted fracture of bones of leg; wound sealed by compound tincture of benzoin; recovery, with good limb.*

(Reported by Dr. E. Hicks.)—Michael M., aged sixty-six, an intemperate cab-driver, was admitted on August 7th, 1871, with a compound comminuted fracture of the left leg. The injury was produced from the kick of a horse, and was therefore direct, the fracture being at the junction of the middle and lower thirds of the limb. A piece of bone projected through a lacerated wound, and the skin was severely bruised, and there was much venous bleeding. The dresser, Mr. Peacock, acting on Mr. Bryant's suggestion, at once reduced the fracture, and closed the wound with several pieces of lint saturated with the compound tincture of benzoin, fixing the limb upon a posterior splint, with two lateral ones, and swinging it. No pain or constitutional disturbance of any kind followed the accident. When the lint was removed on the twenty-fifth day, the wound had completely healed, and the fracture united. On Sept. 15th a stiff apparatus was applied, and on Oct. 3rd the man left the hospital well.

Case 2.—*Compound comminuted fracture of the tibia and fibula; wound sealed with the compound tincture of benzoin; recovery.* (Reported by Mr. Fry.)—William B., aged twenty-four, a warehouseman, was admitted Nov. 25th, 1871, with a compound comminuted fracture of the left tibia and fibula. It had been caused by a weight falling upon the limb and crushing it. The man was carried to the hospital immediately after the accident. The dresser, Mr. Burgess, detected a comminuted fracture of both bones of the leg at the lower third, and a contused wound of the integument covering the bones and communicating with them.

The wound was at once cleaned, and then sealed with pieces of lint saturated with the compound tincture of benzoin. The leg was then put up with a posterior and two lateral splints, resting on a swing. No constitutional or local trouble followed, and the dressing was not disturbed till Dec. 25th—that is, for twenty-seven days after the accident,—when it was taken away, the wound having healed and the fracture consolidated.

A flannel gum-and-chalk bandage was then put on, and the man got up. He left the hospital on Jan. 19th, with a good leg, well.

Case 3.—*Compound fracture of leg; wound sealed with the compound tincture of benzoin; recovery.* (Reported by Mr. St. C. Shadwell.)—Albert G., a drayman, aged eighteen, was admitted on the 13th March, 1872, with compound fracture of the leg at the junction of lower with middle thirds. While driving in a low van, his horse kicked up behind and struck him on the upper part of the right leg. He fell on one side into the van, and was conveyed at once to hospital. On admission he was unable to stand, and lost a great deal of blood from the wound.

The wound was sealed with compound tincture of benzoin, and the leg was put up on a back and two side splints. The patient slept well, and was not at all feverish, but complained of pain about the seat of injury.

March 15th. Temperature 97·5°, and from this time to the 8th April he progressed most favourably. The benzoin dressings were then removed, and the wound was found to have scabbed over; two days later the splints were taken off. Union was complete.

April 18th. The leg was put up in gum-and-chalk bandages extending from the foot to five inches above the knee. He got up on the 22nd, and left the hospital on the 26th.

Case 4.—*Compound fracture of left tibia and fibula; compress of compound tincture of benzoin; recovery.* (Reported by Mr. D. T. Evans.)—I. K., a carrier, aged thirty-two, was admitted on Nov. 15th, 1872, with a severe compound fracture of the tibia and fibula at the junction of the middle and lower thirds of the limb; the bone protruded. He complained of great pain and coldness in the limb. Whilst he was alighting from his cart his foot slipped off the step, and he fell, the wheel passing over his leg. He was quite insensible after the accident, and unable to move his leg.

The day after admission, the wounds of the skin over the fractures were covered with lint saturated with compound tincture of benzoin, and the limb placed on a posterior splint with two side-splints, and swung in a cradle.

Nov. 18th. Pulse 80, regular and strong; temperature 99·6°. He slept badly in the night, on account of violent twitchings of the limb, and he complained of pain in the ankle-joint. He passed his water freely, but it was high-coloured and cloudy. His bowels were confined, and he was troubled with a cough, which gave him great pain about the tenth rib on the right side; he had no expectoration.

19th. Pulse 88; temperature 98·8°. He had great pain in the back, which prevented him sleeping, but he took ten grains of chloral hydrate three times a day. His cough was better, and he had no pain in his leg.

20th. Pulse 84; temperature 99·2°.

21st. Pulse 96; temperature 99·2°.

25th. The cough had subsided, he slept well, and had no pain in his leg, and he took quinine mixture three times a day.

31st. The benzoin dressings were taken off; the wound had healed entirely by scabbing. The splints were removed, and the tibia was found to have united pretty evenly. The patient was quite well in himself.

Jan. 3rd. The leg was put up with gum-chalk and flannel; the union of the fragments was very even, and a slight amount of callus was thrown out.—*Lancet, Nov.* 25, 1876, *p.* 747.

46.—CASE OF BADLY-UNITED FRACTURE OF THE FEMUR TREATED BY SAWING ACROSS THE BONE AT ITS ANGLE, WITH ANTISEPTIC PRECAUTIONS.

By Joseph Bell, Esq., F.R.S., Surgeon to the Edinburgh Royal Infirmary.

Master J. M., æt. 12, sustained, on 19th October 1875, a severe comminuted fracture of his left femur, about four inches below the hip-joint, from a horse falling on him. He was placed in the hands of a well-known bonesetter in the east of Fife, who was summoned by telegram, and who undertook the case on his own responsibility. I am informed that he treated it by bands and splints, without any precaution to prevent movement of hips or knee-joints; and in a soft bed, which allowed the patient's hips to sink into a hollow. He did not examine it for eight weeks after accident. The treatment extended over ten weeks, and during the greater part of this time the patient suffered very severely from pain, feverishness, and sleeplessness.

State of Master M. when first seen by me on 30th June 1876:—

His left lower limb was shortened to the extent of five inches; he had a high-heeled boot, but even with its aid walked with great difficulty. The foot was everted, pelvis twisted, and back had a compensating curve. On stripping him, I found the deformity to depend on a fracture of the left femur, which had probably been comminuted, but which had united, deformed both by an angular displacement, and also by a rotary displacement. The upper end of the enormous mass of new material which mended the fracture was about 4 inches from the hip joint; the thickened mass of bone was about $2\frac{1}{2}$ inches in diameter at its thickest, and projected as an acute angle outwards and forwards. When the limbs were laid side by side, the shortening in consequence of the angular displacement was about 5 inches; and the lower portion of femur, knee-joint, and leg and foot were much everted. The salient angle, which contained less than a right angle, projected sharply under the skin; but the amount of new bone thrown out behind was very great—so much so as to overlap inwards and backwards the femoral artery, which could be easily compressed against the bone.

The boy's general health was good, but his lameness was very bad, and was getting worse instead of better; and his halting gait and spinal curvature were giving much uneasiness to his parents.

I explained to his relations that it would be necessary to do something, and proposed to attempt refracture, though I explained to them that the length of time which had elapsed

since the injury, the immense amount of new bone, and also the high situation of the fracture, would render the operation difficult and doubtful.

They placed the case unreservedly in my hands; brought the boy to town; and on Monday, 14th August, I attempted refracture of the bone, assisted by Dr. Grant and Messrs. Brown and Scougal. Having put the patient deeply under chloroform, I first tried, with all the strength I could muster, to break the bone across my knee—no effect was produced. I then used Mr. Butcher's powerful steoclast, with which I was provided through the kindness of Professor Spence, who lent me his own one, made on Mr. Butcher's model. This powerful screw failed to fracture the bone. The angle being such a salient one put the pressure at a great disadvantage; and after long and exhausting efforts carried on as far as I dared to risk the vitality of the parts, we had to give up, absolutely baffled.

I then explained to the parents by letter the necessity for future operative procedure; and on Monday, 21st August, after putting him fully under chloroform, with the assistance of Dr. Grant and Messrs. Brown and Scougill, I performed the following operation:—

The parts being thoroughly washed and soaked in a solution of 1-20 carbolic acid, I, under the protection of a steam spray, cut down to the angle of the bone, with a strong-backed, straight scalpel, dividing all the tissues, including the periosteum. The incision was rather a stab than a cut, passing obliquely backwards and outwards from a point nearly over the superficial femoral artery down to the bone. On withdrawing the knife I closed the wound with ring finger of the left hand, and maintained the accurate apposition of the edges. Into the wound thus made I then introduced a long, narrow-shaped saw, tapering nearly to a point, and with rapid but very slight movements of it sawed through the bone at its angle as far backwards and inwards as the line of the artery (which I could distinctly feel under my left finger) allowed me. When the bone was thus with some labour nearly divided at its thickest part, I then withdrew the saw, and putting a pad of carbolized gauze over the aperture of entrance, succeeded in fracturing the bone completely across my knee. A little gentle force then soon succeeded in restoring the straightness and nearly the original length of the thigh. The wound was dressed antiseptically, and two Gooch splints, well padded, put on, one on the outside, and the other in front of the thigh. The apparatus for applying extension by weight and pulleys had been already prepared, and the plaster fastened to the leg, knee, and lower third of thigh; so it was at once put on, and we found that

about ten pounds of sand kept the limb in admirable position, without undue pressure or muscular fatigue.

The after-progress of the case was so absolutely uneventful as to leave me nothing to record. He never lost an hour of sleep or a single meal. He never had a twinge of pain in the wound. His temperature and pulse remained normal. I dressed it under spray at first daily, then every two, three, and four days; at length only weekly. The wound healed at once, so far as the deeper parts were concerned; and had it not been for a superficial sore, the result of the extreme force used on the first occasion, the dressings would have been very few. On the 14th of October the splints and weights were removed; and at the end of the next week he went home, using a crutch, within nine weeks of this somewhat formidable operation.

Those who are familiar with current surgical literature and practice will be aware that in the present day there are at least three different plans in common use by which deformed or badly-united bones may be reingrooved:—

1. Subcutaneous osteotomy as proposed and practised by Mr. Adams, in which a strong knife and a narrow saw are used to divide the neck of the femur in cases of osseous anchylosis of the hip-joint in bed position.

2. Osteotomy, also subcutaneous, by chisel and mallet, as advised by Mr. Maunder, and practised by him, Mr. Annandale of this city, and others.

3. Section of the bone and resection of the badly-united ends with antiseptic precautions, as advised and practised by Mr. Lister.

The plan I adopted in this case seemed to me to unite the advantages of subcutaneous and antiseptic surgery. Being subcutaneous, I could almost ensure against entrance of air; being antiseptic, I could prevent putrefaction; by attention to escape of serum, tension was also avoided.

I thought of using chisel and mallet, but preferred the saw as being more manageable and less likely to risk injury of the femoral artery, which was so much in the way. The only really anxious part in the operation was in the breaking of the last bit of the bone, in case of injury of the vessel.

I need hardly say that the position of the fracture within less than five inches of the hip-joint made it very dangerous, had anything gone wrong in the wound, as a compound fracture of the upper third of the femur is well known to be a frequently fatal accident.—*Edinburgh Medical Journal, March* 1877, *p.* 785.

47.—AFTER-TREATMENT OF EXCISION OF HIP-JOINT.

By J. W. Hulke, Esq., Surgeon to the Middlesex Hospital.

[Mr. Hulke, commenting on an interesting paper by Surgeon-Major Porter, says, with reference to the splint,]

I had previously tried most of the well-known apparatus, such as the hammock, bracketed and interrupted splints, weight and pulley, sand-bags, plaster of Paris spica, and had been disappointed with all as suitable and convenient appliances during the first weeks after excision, when ten years since I began to use the splint known by us as De Morgan's; and I have been so satisfied with it that I have from this time, with hardly a single exception, used nothing else. It is easily applied; it allows, if nicely regulated, extension; it is much less cumbrous than a special bed or stretcher (to the merits of which I am not insensible); and it permits the most free access to the wound. My patients are always placed on a horse-hair mattress laid on boards to insure a level bearing-surface. A circular ring water or air-cushion (which should not be too much distended) is put under the pelvis, with the double object of obviating bed-sores and preventing pressure on the wound, which might hinder the free escape of the discharges, which latter is always specially provided for by drainage-tubes.

The splint may be described as a long Liston's thigh-splint, having at its foot-end a cross-bar, in which is a longitudinal slit for a pulley, which can be shifted in it to any required distance. There is another pulley where the short and long pieces of the splint join. The splint is applied to the sound limb, and counter-extension is obtained by a perineal band on this side, whose extension of the operated limb is effected with a cord fastened to a stirrup of plaster, or better to a loop of bandage stitched to the sides of the leg of an ordinary stocking, which clings sufficiently to the limb when secured by a few circular turns of roller. The cord running over the pulleys passes up the outside of the long piece of the splint, and is kept tense by an India-rubber accelerator attached to the upper or trunk end of this. A little perforated slip of wood such as is used for tightening tent-ropes allows easy regulation of the tension.

For dressing the wound or the use of a bed-pan, the patient may be rolled easily towards the sound side, and, if this be done gently, the attendant placing one hand on the iliac crest and the other on the limb to insure their simultaneous and congruent motion, it entails little disturbance or suffering.—*British Medical Journal, March* 10, 1877, *p.* 311.

48.—ON AMPUTATION.

By T. Holmes, Esq., Surgeon to, and Lecturer on Surgery at, St. George's Hospital.

In the performance of the operation itself, I shall notice the following particulars:—1. The advantages or otherwise of using the spray of carbolic acid lotion during the operation. You have seen me use this spray in most operations during the last year or two, and certainly most of them have done very well. For reasons which I shall presently state, I cannot satisfy myself of the basis on which the "germ theory" rests; but I find it difficult to resist the testimony of the numerous surgeons of experience who attach importance to the fact of operating in an atmosphere of carbolic acid; and as in an amputation there is no inconvenience in it, whatever may be the case in a dissecting-operation, I think it better to use it.

Next as to cutting flaps. All the methods in use have their advantages in appropriate cases. When the surgeon can do as he likes, it seems to me that the best stump in the thigh or arm is formed by separating skin-flaps on the anterior and posterior aspects of the limb, making one (usually the anterior) much longer than the other; then making a circular incision through the superficial muscles; then another down to the bone; and, finally, dividing the bone about an inch higher up. Thus the end of the bone is well covered with muscular tissue, and the scar is fairly away from pressure. In the leg and forearm, Teale's method, or some modification of it, seems to give the best results—*i.e.*, the bones should be covered by a good cushion of muscular tissue, and the skin-wound should be placed away from the face of the stump. It is true that the muscular tissue of the stump will probably become atrophied, and ultimately disappear, but in the meanwhile the cut ends of the bones will have become covered with new tissue, and will not adhere to the face of the stump. The objection to the old circular method is, that the scar necessarily crosses, and almost always adheres to, the ends of the bones; while the objection to all those methods in which the scar is placed out of the way of pressure (including Teale's and Carden's) is, that the length of the flaps renders some amount of gangrene probable in weakened or unhealthy persons, and must in all cases render rapid union less likely. You have seen, in the case which gave rise to these remarks, one of the disadvantages which sometimes is noticed when the flap is entirely musculo-cutaneous—*i.e.*, when the skin is not separated from the muscles at all,—or when, as in that case, there is no flap at all on one side. Being anxious to have a short stump in the forearm, which I thought would be more movable and more useful than one formed by amputating

through the elbow-joint, I was obliged to cut perpendicularly down to the bones in front, since the laceration extended to within a short distance of the bend of the elbow, so that the whole covering of the bones was taken from behind. This covering was formed from both muscles and skin, and looked at first too exuberant; but as union has advanced, the contraction of the muscular tissue in front has drawn it so tightly over the end of the radius, that the latter would have made its way through the skin had we not contrived to relax the parts by the application of a splint and strips of plaster.

Next with regard to the stoppage of bleeding. Finger-pressure, the tourniquet, and Esmarch's elastic bandage, are all perfectly adequate to guard against any alarming hemorrhage in ordinary cases; in fact, alarming hemorrhage is very rare. In looking over the notes of 500 completed cases of amputation at this hospital, I find a few cases in which death occurred on the table immediately after the completion of the amputation, but in none of them was there any unusual hemorrhage; and hemorrhage so copious as perceptibly to affect the progress of the case is, I think, very seldom observed. It seems to me, however, that, with ever so steady and experienced an assistant, the patient generally loses more blood with finger-pressure than with the tourniquet, and especially in amputation of the leg. The recently introduced bandage of Esmarch is a most convenient apparatus for dissecting-operations in the limbs, and is, I think, an improvement in many cases of amputation. You saw me amputate a child's thigh, two or three weeks ago, after this "bloodless" method, and you could see how well in that instance it deserved its name—for hardly a spoonful of blood was shed. It is true that this is not always the case. The vessels will ooze for a time after the removal of the bandage; and this oozing sometimes causes about as much loss of blood as would have occurred with the tourniquet. The main utility of Esmarch's bandage is in cases of excision, of operations on bones, and of removal of tumours, particularly when deep-seated—cases in which it may be of the most vital importance that the surgeon may be able to see all the tissues as clearly in the living as in the dead subject. The disadvantages which have been attributed to the use of this apparatus I have never seen—having had a tolerably extensive experience of its use. I have never met with recurrent hemorrhage afterwards; and this I attribute to free exposure of the surface of the wound to cold (by means of carbolised lotion) till all tendency to hemorrhage is over. Nor have we ever seen the least tendency to gangrene, though some of our operations have been protracted. The idea that pus may be diffused over "the cellular spaces" of the limb by the pressure is, I think, wholly theoretical—at least,

I know of no case which supports it; and it is improbable, since there are no cellular "spaces" in reality. What we are in the habit of calling "the cellular tissue" is more properly described by the older name of "the cellular membrane;" it is a continuous membrane, the fibres of which can indeed be distended into cellular spaces, but no such spaces exist in the natural condition. Nor do I believe that the pressure of the bandage is at all sufficient to inject pus into its interstices, for we must remember that the pressure need not be at all severe. It is its constant, gentle action which empties the limb of blood. The attempt which was made on the first introduction of Esmarch's proposition to represent it as identical with the old plan of applying a bandage before putting on the tourniquet showed a complete misapprehension of the matter. A tourniquet always detains the blood in the limb, and though a little less may be detained if the limb has been previously raised and bandaged, still the limb will always be found tolerably full of blood. The pressure of Esmarch's circular elastic cord, on the contrary, is sufficient to prevent any supply of blood to the limb, while the capillaries and veins have been previously emptied by the gentle continuous pressure of the bandage, so that all the tissues are bloodless, while with the tourniquet they are loaded with blood. I have nothing but good to report of Esmarch's plan, though I do not think amputations are the operations best adapted to display all its advantages.

Next of the vexed question of ligature or torsion. We have ceased at this hospital to use acupressure, because, after a satisfactory trial, we thought it decidedly inferior to the old plan of the silk ligature, and some of my colleagues still use the latter. I have, as a general rule, abandoned it for the carbolised catgut ligatures, which I have found equally efficient against primary and secondary hemorrhage, and which offer (I am again speaking only generally) no obstacle to the rapid union of the wound. You saw this illustrated in the case of the little child whose thigh was amputated. The wound was healed almost entirely, and the patient out of bed, on the fourteenth day after the amputation, no trace of the ligatures having been seen. I prefer this plan to torsion (though quite admitting the efficacy and safety of torsion in skilful hands), inasmuch as it is very much easier to apply, and involves much less handling and contusion of the parts. In an amputation in which torsion is used, though the larger and more obvious vessels may be easily and quickly secured, we often see much difficulty in dealing with those which come less distinctly into view; and this difficulty is greater if the tissues are unhealthy. In such cases either the parts are left long exposed and are much handled —which cannot but retard union—or the smaller vessels are left

to bleed; and even if this bleeding ceases without any serious damage to the patient, yet the blood must distend the flaps, some of it must remain as clot, even in spite of drainage, and it must in that way again retard union. With the carbolised ligature each vessel can be permanently secured in a moment; and the small knots of gut left in the wound do not seem in any way to affect the rapidity of its union. It is for these reasons that I prefer this form of ligature in amputation. I have used torsion occasionally, and, I repeat, am perfectly convinced of its efficacy; nor have I myself had any ill consequences in the few cases so treated. In one instance of forearm-amputation in the practice of one of my colleagues, severe secondary hemorrhage occurred on the seventh day, from one of the main arteries, but this would probably have occurred also if the ligature had been used. It forms, to my mind, no argument against torsion, and it may be balanced by a case of my own, in which fatal secondary hemorrhage occurred from the femoral artery on the eighth day after ligature with carbolised gut—the only case of secondary hemorrhage I have had or know to have occurred with this form of ligature. And here, again, there can be little doubt that the bleeding would have occurred in whatever way the vessel had been secured. It was a case of secondary amputation, performed on account of abscess in the knee-joint from injury. The patient had high fever before and after the amputation. Five days afterwards he had convulsion-fits, in which he nearly died; and on the eighth day suddenly fatal hemorrhage came on. The carbolised catgut ligature had entirely disappeared, and the artery was perfectly empty and open. Now, it is quite clear that if the pressure of an acupressure needle for forty-eight hours is sufficient to close the popliteal artery so firmly that, under ordinary circumstances, no hemorrhage is to be apprehended, the pressure of the carbolised gut during the whole time that its knot remains firm (a period which can hardly be estimated at less than five days) must be more than enough if the blood have its usual tendency to coagulate. But the unhealthy state of the blood in this instance was shown by the fever before operation, and the convulsions afterwards. Now, in cases where the blood will not coagulate in the vessels, hemorrhage will probably ensue after either torsion or acupressure, and is still more certain to come on after the ulceration by which the silk ligature is cast off.

The arteries having been secured, and all oozing from minute vessels commanded by the affusion of cold carbolised lotion, the wound is closed. No part of the "antiseptic" method seems to me so important as its insistance on thorough drainage. And I am sure that the rapid union of the greater part of the stump may generally be secured, and the patient thus rescued from

much of the subsequent danger, if, by means of a drainage-tube introduced at the bottom of the cavity, and brought out at either end, a drain is established, by which all painful tension of the stump is obviated. All the rest of the wound may be united by sutures; and the greater part of it, at any rate, will often heal by first intention. I always use silver sutures, as I am in the habit of leaving them untouched for many days—in some cases even till the patient is ready to leave his bed; whilst carbolised sutures dissolve and break after a few days.

Amputation being over, the wound has to be dressed, and in my opinion that form of dressing is the best which necessitates the least handling of the parts, and insures the greatest amount of quiet, thorough drainage being always understood. But though these general principles would be admitted by most surgeons, the methods of dressing are endless. I should like to call your attention to a very interesting paper, "On the Treatment of Amputations by the Open Method," published by Dr. Dennis, House-Surgeon of the Bellevue Hospital, New York, in the New York Medical Journal, January, 1876, in which he details fourteen successive unselected amputations of all kinds in the practice of Dr. James Wood, of that city, which all recovered. In that method "the flaps are not even approximated, but left entirely open," the stump being supported on a pillow of oakum—the discharge removed by frequent irrigation with carbolised lotion, and balsam of Peru poured over the granulating surface. When suppuration has nearly subsided the flaps are gradually approximated by plaster. The advantages claimed for this method are the thorough drainage which it necessitates, and the absence of any putrefiable material between the flaps, to the retention of which the author ascribes most of the disasters of amputation. As a consequence of this, the degree of consecutive fever is said to be much lower than after amputations treated by closing the flaps, and its duration less. The method must be allowed to be a good one, as shown by so long a series of successful cases, which, however, Dr. Dennis is in error in calling "unprecedented," for many longer series have been recorded under different methods of treatment—notably Mr. Callender's. Still we must allow the merit of the method if compared (as the author compares it) with that by total occlusion of the flaps, drainage being entirely neglected. But if compared with any form of dressing in which the flaps are brought together, but effectual drainage is established, I cannot say that I see any proved advantage in this over methods which aim at more rapid union. Dr. Dennis says:—"In all the cases operated upon for amputation, and treated by the closed method, in this hospital within the past year, there can scarcely be found a patient whose [maximum?] temperature

was as low as 103° Fahr. In the major amputations the thermometer has shown a record of 104° Fahr., and, in many cases that have been carefully examined, even about 105° Fahr., and in quite a number even as high as 106° Fahr." This is totally opposed to our experience of the method I have recommended, in which, as far as I remember, I have never noted the temperature higher than 103°, even after thigh amputations, except in cases of pyæmia, on which subject I will speak presently. I conclude that if due provision for drainage is made, the approximation of the rest of the flaps accelerates healing, and improves the shape of the stump. The best stumps, as far as I have seen, are those in which the edges unite most rapidly. As one of the essentials of success in amputation, I would specify rest of the stump. It is most important, not only for the comfort of the patient, but for his safety also, as I believe, to protect him from those spasms which indubitably disturb the parts and distress the patients, and probably aid in setting up fatal inflammatory action. To this end the gentle, equable support of a splint evenly bandaged on to the stump seems to me most useful.

Acting on these general ideas, I always use sutures to support the flaps, and leave them in for a time which is practically unlimited. Even when they act, if only to a slight extent, I see no object in withdrawing them; for if they still afford moderate support to the flaps, they will avoid the use of strapping, and thus diminish the handling to which the stump is subjected. I am aware that many very good surgeons dress their stumps chiefly with strapping; and in the practice of Mr. Hewett you have had opportunities of seeing amputations treated without any suture from beginning to end. And so, in the "open method" just described, sutures are rarely used. I only speak of what seems to me the best among several good plans of treatment.

In considering the dressing and after-treatment of the case, we must discuss the value to be ascribed to antiseptics, and this connects itself with the all-important question of the exciting causes of pyæmia. Without going into details, for which I have no time here, I may lay it down broadly that the death-rate after amputations of about equal grades of severity will vary with the prevalence of pyæmia. Many patients, of course, die from the shock of previous accident, or the exhaustion of previous disease, but such considerations I class as coming under the head of "the severity" of the operation; for an operation, such as a pathological amputation of the forearm, which would involve hardly any danger at all to a boy in moderate health, may be a proceeding of extreme severity, and very probably fatal, in the case of an old man exhausted by

visceral disease. Excluding this consideration for the moment, we can only hope to diminish the risk of amputation by diminishing the prevalence of pyæmia, and of those affections —such as erysipelas, diffuse cellulitis, absorbent inflammation, and phagedæna—which have a strong affinity for pyæmia, own probably, in many cases at least, the same causes, and when they prove fatal usually do so by giving rise to definite pyæmia. Now, on the subject of the causation of pyæmia we have four different theories to discuss—(1) that it is caused by the implantation on the wound of germs which are present in the atmosphere; (2) that it depends on unhealthy inflammation generated in the wound and the vessels leading from the wound by a diseased condition of the patient's system; (3) that it depends on previous disease or injury of the parts concerned in the operation; and (4) that it is inoculated on the wound by putrefying or putrefiable matters carelessly introduced in the dressings. I need hardly say that none of these theories excludes any of the others. They may all be true in different cases. But if we look carefully at the first theory we cannot but feel great difficulty in admitting it, notwithstanding the high surgical and still higher philosophical authority on which it rests. That the atmosphere of a ward or sick-room may be poisoned by the accumulation in it of a number of acutely suppurating wounds or by other impurity, and that under such circumstances pyæmia and other allied affections may attain an even frightful prevalence, is very true; but this is far from proving, as Professor Tyndall would have us believe, that the cause of pyæmia resides in the minute dust-cloud, which is always demonstrable by a powerful beam to be floating in the air. Stated in this naked way, I think we can almost confidently say that the theory cannot be true. For the first leading fact in operative surgery which we notice with respect to the sequelæ of operations is the remarkable immunity of children from all such unhealthy conditions. And how can that be reconciled with the idea of atmospheric contagion? Are not children peculiarly liable to atmospheric contagions of all sorts? And is not this liability rather increased than diminished in regard to those contagions which are unquestionably material? How, then, can we explain the undoubted fact that the prevalence of pyæmia after amputation (and I have no doubt the same thing is true of all other operations) advances steadily with each decade of life, so that, apart from the risk of shock or exhaustion, amputations of the gravest character, such as primary thigh amputations, can be performed with almost a certainty of success in infancy, while they become more dangerous in youth, and are nearly certainly fatal in advanced age? If the supporters of the germ theory reply that in childhood the soil is, as it were,

not prepared for the growth of the seeds deposited upon it from the atmosphere—*i.e.*, that the tissues of children do not readily take on the actions which lead to pyæmia,—many facts would lead us to doubt this explanation, which in any case is merely conjectural. Such facts are—the somewhat great prevalence of erysipelas in childhood, and the readiness with which acute periostitis and acute osteomyelitis are generated by trifling injuries at an early age. These diseases have a great analogy with pyæmia, and very frequently terminate in secondary deposits in the viscera. Again, the sporadic manner in which pyæmia shows itself in a well-managed hospital like this is a very serious difficulty in the way of the germ theory. If the seeds of pyæmia are always floating about our wards in clouds, how comes it that the growth is not developed in one wound out of a hundred? The most exaggerated impressions prevail, I am sure, in medical circles, and still more among the laity, about the prevalence of hospital diseases (so called) in our metropolitan hospitals. I have frequently met hospital governors, and even medical practitioners, who have received with obvious incredulity my statement that I often go weeks and sometimes months without seeing a case of erysipelas or pyæmia, and that, as far as I can see, our hospital is quite as healthy as any private house, in the sense of being as safe a place for the treatment of an injury or operation. Yet such is the fact. Dr. Bristowe and I showed long ago that, taking a period of about two years, in three of the largest metropolitan hospitals (viz., Guy's, St. Thomas's, and St. George's), the deaths from pyæmia were only 1·23 per cent. of the total number of deaths, and only 0·115 per cent. of the total number of patients, or that, roughly speaking, but one patient in a thousand of all those admitted (medical and surgical) died of pyæmia—a disease, be it remembered, usually fatal, and which prevails in medical wards as well as (though of course not so much as) in surgical. Again, Dr. Bristowe and I found during our visits to all the metropolitan hospitals that less than 2 per cent. of the surgical patients were at that time affected by all the (so-called) hospital diseases put together; and this total is of course made up chiefly of cases of erysipelas in its various forms, and of those most were admitted with the disease, and many had no wounds at all. Could this have been the case if these diseases are sown broadcast on all the wounds in our hospitals by clouds floating continually in the air? Nor can we leave out of sight the great success which attends other systems of dressing in which the distinctive principles of the antiseptic method are either neglected or expressly violated. I have already alluded to the results of the open method of treatment in the hands of Dr. James Wood, of New York, and if this experience is objected

to (as perhaps it fairly may be) on the ground of the small number of cases—though the uniform success of fourteen unselected consecutive amputations of the limbs is a striking testimony to the harmlessness, to say the least, of the treatment,—no such objection, I should think, can lie against the experience of Dr. Humphry, of Cambridge, who has, I believe, treated all or most of his capital operations for many years on the "open" method, combined with drainage. This method, of course, violates the cardinal principle of the antiseptic theory —viz., that for the safe treatment of large wounds the air must be filtered through a medium in which its noxious germs are destroyed. And the exceptional success of Mr. Callender in amputations has been obtained under a system of treatment in which, although carbolic acid is used as a dressing, none of the special precautions said to be essential to the success of the antiseptic method are employed. Nevertheless, the results obtained by these surgeons are, as far as we know, equal, if not superior, to those which have followed the strict employment of all possible antiseptic precautions. Yet although, for these and other reasons, I cannot accept the theory in its entirety, and though I give myself no trouble in following out the practice in its minuter details, I would not have you think that I am insensible to the merits of the treatment which Mr. Lister has introduced, or that I have been slow in attempting to avail myself of its numerous advantages, or rather, I would say, in trying to realise these advantages for my patients. And I think I may say that the results have been satisfactory. To test this, I have looked down the records of our Amputation-book since the commencement of the year 1870, which, as far as I can recollect, is about the date at which I began to treat operations on the modified antiseptic method which you see me employ. I find attributed to me thirty-two amputations of all kinds, with about the usual proportion of primary, secondary, and pathological amputations of the various limbs (partial amputations of the hand and foot being excluded), and, of course, comprising the natural variety of age, sex, and state of health. Out of these thirty-two cases four died. But one of the fatal cases was an amputation at the hip in a child, after the failure of excision (delayed, in consequence of the opposition of the parents, till there was no reasonable prospect of recovery, and in which I think I ought not to have amputated). The child sank in a few hours. Another was a case of secondary amputation at the shoulder-joint, at the age of seventy, for gangrene, the result of displacement of a fragment in a fracture of the neck of the humerus, which had ruptured a diseased artery; and here the patient died on the seventh day of secondary hemorrhage. The arteries in this case were tied with silk, and no

antiseptic precautions were adopted. Both the other fatal cases were secondary amputations. One of the thigh, which proved fatal by secondary hemorrhage, I have above referred to. The other, which was a case of amputation of the forearm for gangrene after a wound involving the main arteries and nerves of the hand, is the only death that I have had from pyæmia after amputation in the whole series, and in this case I think it very probable that the pyæmia dated from before his admission into hospital. But the above list does not include two cases of amputation of the leg in which the operations were performed not in this hospital, but in our country branch at Wimbledon—the only two amputations which have been performed there, and both of which proved fatal: one of pyæmia, the other of erysipelas of the head and face. Although treated in separate rooms of ample size, which had never been used before, in a magnificent country situation, with the services of a separate nurse, and with "all the appliances and means to boot" that a monarch could have had, these patients died of blood-poisoning a few days after the operation. I need hardly say that so disastrous a commencement indisposed me to persevere with the experiment of amputations in the country, especially as the result of those performed in our London hospital is so satisfactory. I ought to add that in neither of these cases were any antiseptic precautions taken, nor was carbolic acid used in the dressings. I hope I have shown you some reasons, satisfactory to a practical surgeon, if not to a professor of physics, for withholding assent to the now fashionable germ theory, and also some reasons which have borne the test of considerable experience (of which this series of amputations is only a small part), for, nevertheless, adhering to the above system of dressing, seeing that it appears to combine all the essentials of good treatment of wounds—viz., perfect drainage (which, I cannot too often repeat, seems to me the first requisite), equable support, entire rest, and an absence of all risk of contamination from any accidental impurity. Its only drawback, as far as I have seen, is the somewhat irritating quality of the acid, assisted by the heat kept up by the environment of the wound. I have often found myself obliged on this account to renounce the use of the carbolised dressings after a fortnight or three weeks, by which time, however, all serious danger is usually over. This irritating quality of the dressing seems to be the motive of many of the endless modifications made in its details.

Now to proceed to other theories of the generation of pyæmia. That the state of the patient's health, and the previous condition of the tissues through which the operation is performed, have much more to do with predisposing the patient to pyæmia after

amputation than all other causes put together, is to me more certain than any other fact in operative surgery; and yet it is strange how little this fact is dwelt upon in the treatises on hospital hygiene. If you read these authors, without any practical experience, you would believe that the chief danger in an amputation is that it should be performed in a hospital where the cubic or the floor space is too limited or the ventilation imperfect. If you come to practise surgery in a large hospital, you will see that the chief danger in amputation is that it should be performed on a patient weakened by disease and intemperance, and in tissues broken down by previous diffuse inflammation, whether called erysipelas, cellulitis, angeioleucitis, acute abscess, or gangrene. I do not mean to say that you will be led to undervalue the real effect of good or bad hospital arrangements, but you will see that, if proper care and cleanliness are enforced, the details on which, a few years ago, such stress was laid, are of very minor importance, and that no changes made in them could materially affect the success of our operations.

Time would fail me if I endeavoured to accumulate the proofs which our Amputation-book would afford of the fact that the danger of amputation (measured, as it is, by the danger of the occurrence of pyæmia) depends, above all other things, on this—whether the amputation has been immediately preceded by severe irritative fever ("surgical fever," as Sir J. Simpson called it), or by the efficient causes of such fever. And I hope it is needless for me to furnish you with such proofs (though nothing would be more easy), since I cannot conceive of the mental condition of any practical surgeon accustomed to amputation who could deny it. To take a flagrant instance:—Two men are admitted, equally young, strong, and healthy, sufferers from the same accident, and placed in neighbouring beds in the same ward. In one the leg is hopelessly disintegrated; in the other, not. The former suffers from traumatic gangrene, but amputation is delayed. As the gangrene advances, diffuse inflammation proceeds up the limb, and especially along the course of the absorbent vessels. At last the surgeon makes up his mind to interfere; but, too probably, in vain. Wherever and however the case is treated, blood-poisoning is only too likely to ensue. Meanwhile, the other man, though exposed to the same atmosphere, and treated on the same principles, has remained free from any serious constitutional disturbance; and if any local mischief should, in the surgeon's judgment, necessitate amputation, it may be performed with every prospect of success. Or look at the difference between the dangers of amputation performed immediately on the subsidence of phlegmonous erysipelas, when the surgeon thinks

the removal of the limb absolutely necessary to give the patient a chance of life, and those of the same operation if performed long afterwards to rid him of a member which is a useless encumbrance. In the first case, pyæmia is (to put it much below what I believe to be the case) ten times more likely to follow than in the second. Or take the case of a primary amputation. Is not the avoidance of blood-poisoning far more probable when the amputation is performed through healthy than through contused or lacerated parts? I allow that many cases of primary amputation prove fatal by pyæmia in which the surgeon has made his flaps through healthy structures; but I would submit to further experience how far this may have been due in past times to the neglect of efficient drainage. I think there is the best reason to hope that the frequency of pyæmia in these cases may have depended on retention of putrefying matter in the deep parts of the wound. I know that in old times, before drainage was thought of, or when, as was taught —most incorrectly, as I believe—the ligatures were left to serve as a drain (whereas they generate an amount of suppuration in the deepest parts of the stump which they are quite inadequate to carry off), we used constantly to see the flaps become so tense a few days after the amputation that the surgeon was obliged to cut several of his sutures. This is very rarely necessary, I think, if drainage is secured and the stump treated antiseptically. But much more extensive experience than I possess at present is necessary before any decided opinion can be pronounced on this point.

Finally, what shall we say about the theory that the cause of pyæmia is direct inoculation, as by careless dressing? That it is true in some cases I cannot doubt. Direct experiment shows its possibility; and hospital experience seems to me to prove that pyæmia is undoubtedly so produced in some cases. By "hospital experience" I mean this — that whatever form of dressing you adopt, you will find the prevalence of pyæmia less, the more careful you are to observe the minutest precautions in its application. And herein consists, to my mind, another great virtue of Mr. Lister's system. In the "open" method your wound may possibly be tampered with. If the ward is full of the products of acutely suppurating wounds, some of the particles may be implanted on it. But when securely swathed in the gauze and macintosh, it is at least safe against any interference till you see it again.

My whole directions for the after-treatment of an amputation may be summed up in three words—viz., rest, drainage, and cleanliness.—*Medical Times and Gazette, Jan.* 6 *and* 13, 1877, *pp.* 3, 29.

49.—ON THE TREATMENT OF WOUNDS.

By Sampson Gamgee, Esq., F.R.S., Edin., Surgeon to the Queen's Hospital, Birmingham.

[We generally learn most about principles of treatment from the simplest cases. A "little old gentleman" consulted Mr. Gamgee on account of a cystic tumour about the size of a hen's egg, in the right temporal region.]

The skin was very red, tense, and painful, and the hat, though a very soft one, was worn with much difficulty. After transfixing the growth vertically through the base, and peeling out the two halves of the cyst, with its bread-sauce-like contents, I dried the interior of the wound with a fine sponge. The edges were then very accurately approximated, and kept so with a few strips of lint soaked in styptic colloid. A few turns of bandage completed the dressing. When I removed it, at the end of five days, there was not a drop of discharge, adhesion was perfect, and afforded a simple but complete illustration of the surgeon's first intention in treating wounds—to secure direct union. All that is visible of the cicatrix is a very fine, pinkish line, extending upwards about two inches from the right ear.

Please to note—firstly, that the wound was thoroughly dried with a fine sponge; secondly, styptic colloid was used to keep the edges in contact; thirdly, the parts were not disturbed until the fifth day, when union was complete and solid.

Drenching wounds with water during an operation, and washing them with it afterwards, are mistakes. Water favours decomposition, which is the enemy of healing action.

The styptic colloid, used to keep the edges of the wound together, is the admirable preparation introduced in 1867 by my friend Dr. B. W. Richardson. (Styptic colloid, as prepared after the instructions of Dr. Richardson, by Messrs. Robbins and Co., of Oxford-street, is produced by saturating ether entirely with tannin and a colloidal substance, xyloidine, or gun cotton, a little tincture of benzoin being finally admixed.) In removing the styptic colloid dressing common water should be scrupulously avoided, and a mixture of alcohol and ether employed, or equal parts of absolute alcohol and distilled water, warmed to a little above the heat of the body.

It has been noted that the dressing was not touched for five days after the operation. Once divided parts—be they hard or soft, bones or muscles, skin or nerves—are adjusted with a view to union, the less they are disturbed the better.

A case illustrating the same principles, though on a somewhat larger scale, is that of C. H., aged forty-three, who was lately in Ward 5, whose right breast I removed on the

20th May, with a small hard gland from the corresponding axilla. Of the operation it only need be said that, according to my usual practice, I cut down upon the sternal origin of the great pectoral and dissected it clean, so as to make sure of thorough removal of the diseased mass. I am convinced that many so-called rapid recurrences of cancer are only growths of pieces left behind, and that *thoroughness* is the very essence of success in extirpation of malignant growths. After removal of the breast, the edges of the wound were neatly brought together by numerous points of silver suture, and dressed with a layer of fine cotton wool and over it picked oakum. An evenly compressing bandage was then applied round the chest, and made to include the arm and hand in the flexed position so as to fix them immovably to the side. The first night the temperature rose to 101·3°, but it never rose afterwards above 100°.

The wound was first dressed at the end of the fifth day after the operation. A great part of it being healed by the first intention, a large number of the sutures were removed, and strips of adhesive plaster applied, so as to keep the edges in apposition; a pledget of oakum with a compressing bandage completed the dressing. On June 1st the remaining sutures were removed. The wound was then nearly all healed, and the same dressing applied. On June 3rd (fourteenth day after operation) the entry on the card is "Patient dressed (as before), and sent home well."

The points in this case to which I wish to direct your attention are—(*a*) the numerous sutures; (*b*) the cotton wool and picked oakum dressing; (*c*) the compressing bandage; (*d*) the rare dressing.

Metallic sutures so very rarely cause any irritation, that they may be inserted very near each other with impunity. Sutures far apart, with gaping intervals, are comparatively useless. If the cut surfaces are to adhere they must be brought into contact and kept there, and for this purpose metallic sutures half an inch apart, or even less, are most efficacious. I often apply intervening strips of lint soaked in styptic colloid, but in this case only placed over the wound a layer of fine cotton wool, and a pledget of picked oakum. The best cotton wool for surgical dressings is that sold for jewellers in thin sheets, about eighteen inches by twelve, with alternate layers of tissue-paper. (It is supplied by S. Jevons, Worcester-street, Birmingham, at 2s. 9d. the lb. parcel.) You will often see claims of priority for cotton wool dressing. I do not pretend to say who first introduced it, but the credit of generalising its application in the treatment of a great variety of surgical injuries chiefly belongs to Burggraeve, of Ghent.

It has been recorded that the breast case was only dressed three times, in the fortnight which elapsed between the operation and the patient's discharge, in accordance with the principle of infrequent dressing, of the minimum of disturbance to insure the maximum of rest, dwelt upon in the preceding case, and equally borne out by those to be presently brought to your notice. To the same end the smoothly and lightly compressing bandage round the chest very powerfully contributed. Of all surgical agencies none so beneficent as compression, none requiring more delicate manipulation, none so inadequately appreciated. Under a smooth and uniformly, while lightly-compressing bandage, extravasations of blood are absorbed, the healing action is promoted, and a soothing influence is exercised. There must be no constriction—only equable adaptation of surface to surface with the light pressure which always comforts. There must be no squeezing like that of an old college friend's hand when seen after long absence; such pressure as that, if continued, is intolerable constriction. The soothing surgical pressure is like that which you interchange with the hand of a lady, the pleasure of whose meeting is tempered by the respectful regard which she inspires. Your hand adapts itself to hers, and gently presses it wherever it can touch it, but nowhere squeezes it for fear of offending. Such pressure as that, when employed by the surgeon in the treatment of injuries, always soothes and heals.

To apply a nicely compressing bandage well, you must practise hundreds and hundreds of times, bearing in mind that in surgical, as in all art, the greatest results are often obtainable from the simplest means, provided they be employed with the skill which can only result from the most patient assiduity.—*Lancet, Dec.* 23, 1876, *p.* 885.

50.—THE OPERATIVE TREATMENT OF GENU VALGUM.

By Dr. Alexander Ogston, Surgeon to the Aberdeen Royal Infirmary.

The following history illustrates, in a bad case of knock-knee, a means of treatment hitherto untried, but which yielded on both legs results so perfect as to warrant its being recommended in similar cases.

The patient, D. G., a male, aged 18, was admitted into the Aberdeen Infirmary on the 19th April 1876. His occupation was that of a moulder in an iron foundry, and his account of the commencement and progress of his disease was as follows:—When six years of age, he had an attack of typhus fever, followed by general weakness, most marked in his lower limbs.

After a time he noticed a deformity in his knees, which became bent inwards, and the deformity has been steadily increasing up to the present time. Until his tenth year he remained pale, delicate, and weak, but with no definite ailment save in his knees, and without any noticeable deformity elsewhere. Since his tenth year his general health has been very good.

Appearance on Admission.—His general appearance is that of a robust, strongly muscular, and well-nourished lad, but whose growth has been stunted. The deformity of his lower extremities immediately attracts attention. When upright, or lying on his back with the limbs straight, the knees fall inwards so as to knock against each other, and one knee has to be placed behind or before the other, to enable him to stand. If his feet are placed together, the right knee crosses completely over to the left knee, and *vice versa*, so that a hand can be passed through between the transposed knees. When he walks he drags the posterior knee from behind its fellow, makes it circumnavigate the latter, and finally come entirely in front of it, before the weight of the body is transferred from the one to the other. The normal slightly outward direction of the tibia is so exaggerated that, when lying on his back, the right leg, in the extended position, forms an angle of 150° with the right thigh, and the left leg an angle of 160° with its thigh. When standing the deformity is greater, owing to the superincumbent weight. In each femur the internal condyle is much elongated, while the external condyle is of normal length, but has its anterior surface so far absorbed that the patella is no longer retained in position, but is displaced outwards, and rests on the outer surface of the femur, with its inner and outer edges directed respectively forwards and backwards. Both patellæ, especially the right, can be pressed inwards to near their normal positions, but immediately slip outwards again. Both lower limbs seem shorter than normal, and on standing the biceps tendon is abnormally rigid and prominent in each.

When the knees are flexed to somewhat more than a right angle, the deviation outwards of the legs completely disappears, and it is easy to recognise by sight and touch that the internal condyles are elongated to between half and three-quarters of an inch. In this position, too, the patellæ are drawn still further backwards, so that they come to rest, not on the femur, but on the outer tuberosity of the tibia; and on the front of the femur the groove between the two condyles can be felt.

There exists a knotty thickening of the lower extremities of the tibiæ, fibulæ, radii, and ulnæ. The clavicles are unusually curved. The upper part of the sternum is very prominent, and the anterior ends of the ribs are enlarged close to their articulations with the costal cartilages. The spine is somewhat

excurved in the lumbar region. The health is otherwise perfect. This deformity was treated for some time by splints and bandaging, until it became evident that such means were utterly futile, and that nothing but an operation could straighten the limbs.

In a somewhat similar case, Mr. Annandale had recently, in the Edinburgh Infirmary, opened the joint and sawn off the elongated part of the internal condyle under carbolic spray and antiseptic precautions. In the above case, however, it was decided to avoid the large opening into the joint, and substitution of a sawn surface for a smooth cartilaginous facet (both unavoidable in Mr. Annandale's operation), by making subcutaneously an oblique division of the elongated condyle, fracturing it off as it were, so that it might be pushed higher up into its normal relation to the outer condyle, and there allowed to unite. It was plain that this should permit the limb to become at once straight, since the elongation of the condyle was, as it were, the essence of the deformity. The following operation was therefore decided upon, and put into practice, on the 17th May 1876, on the left knee:—

The patient was chloroformed, the knee flexed as far as possible, and the thigh turned outwards. A long and strong tenotomy knife (Adams's) was introduced through the skin, three and a half inches above the tip of the internal condyle on the inner side of the thigh, and so far back as to be opposite the ridge of bone running between the linea aspera and the condyle. Its blade was carried forwards, downwards, and outwards, over the front of the femur, with its cutting edge directed to the bone. When its point could be felt under the skin, in the groove between the condyles where the patella would normally have been lying in the flexed position of the limb, the cutting edge was pressed against the bone, and the soft parts and periosteum divided by one slow firm movement in withdrawing the knife. The external wound thus made was about one third of an inch long, and formed the entrance to a subcutaneous tunnel, running obliquely over the front of the femur, and ending in the cavity of the joint. Adams's saw for subcutaneous division of the neck of the femur was introduced into the tunnel, and the condyle sawn off by directing the edge of the saw straight backwards. The position of the saw could be exactly controlled by feeling its point working gradually backwards in the groove between the condyles. As soon as it was estimated that the condyle was almost entirely separated, and that the saw had arrived near the popliteal space, the saw was withdrawn. The knee was completely extended, and then with the hands, and the operator's knee as a fulcrum, the patient's knee was forcibly straightened by bending the leg

inwards. The remaining connexions of the condyle with the femur gave way with a crack on the application of very moderate force, and instantly the leg became as straight as a healthy limb, and could even be put into a somewhat bandy (genu varum) position. *The whole of the operation was conducted under carbolic spray, with a minute observance of Lister's antiseptic precautions, and the limb was put up in antiseptic dressings.* It was then simply bandaged to a Liston's long splint, the thigh and leg being kept in the same line by pads, and the boy was placed upon a hard mattress.

The reaction was almost *nil.* His temperature carefully taken, never rose above 99·8°. The joint at first filled with blood, never became hot or tender, and on the 2nd June, the splint and bandages were discontinued and passive motions commenced.

On the 6th June, the other knee was operated on in the same manner, with an equally favourable result, the temperature never rising above 100°. The splint and bandages were removed on the 21st June, and passive movements commenced.

The movements were continued on both limbs for a month, and were at first attended by crackling and grating, as if from rough or displaced cartilages, whenever the knees were flexed to a right angle. There was never any pain, and the movements became steadily smoother until, on the 9th July, seven and a half weeks after the operation on the left knee, and less than five weeks after that on the right knee, he was allowed to rise and walk. The movements speedily became normal, and he was dismissed, walking perfectly, on the 21st July. The patellæ were even then tending to assume a more normal position.—*Edinburgh Medical Journal, March* 1877, *p.* 782.

51.—NOTE ON THE TREATMENT OF RUPTURE OF THE LIGAMENTUM PATELLÆ.

By John Chiene, Esq., Assistant-Surgeon, Edinburgh Royal Infirmary.

J. F., aged forty-four, was admitted to the Surgical Clinical Wards on the 8th of September 1876. He limped into hospital, complaining that something had given way in his right knee. He stated that, shortly before his admission, in going down a ladder his right foot caught on the last step, and that he fell forwards, his right leg bending under him. On examining the joint, the nature of the accident was at once evident. The patella lay on the anterior surface of the femur above the condyles; there was a distinct gap between the patella and the tubercle of the tibia. The ligamentum patellæ could not be felt. The patient could not extend his leg.

Treatment.—An oblong piece of strong extension plaster, large enough to cover the anterior and lateral aspects of the thigh, was heated, and fixed in position by means of a roller bandage. It was shaped so as to embrace the patella, and to its distal corners, on either side of the knee-joint, two pieces of strong india-rubber tubing 8 inches in length were attached by tapes. The limb was then laid on a posterior splint with footpiece, and slung in the inclined position to a wire cradle. The india-rubber tubing was fastened by means of tapes to the footpiece, and tightened sufficiently to bring the patella to its normal position. The elasticity of the india-rubber relieved the feeling of rigidity, and its contractility counteracted any loosening of the apparatus due to stretching of the tapes or slipping of the plaster.

The patient was in bed eight weeks. The plaster required renewal once. He never had any uneasiness from the apparatus, and was discharged on the 4th of November with firm union. Measurement from the upper border of the patella to the tubercle of the tibia was 4 inches on both limbs. On the 3rd of January 1877 the patient was shown at the Medico-Chirurgical Society, with complete restoration of flexion and extension at the knee-joint. The measurement from the upper border of the patella to the tubercle of the tibia was now $4\frac{1}{2}$ inches, slight stretching of the newly-formed material having taken place since 4th November.

I have also used this simple method in a case of fracture of the patella with an equally satisfactory result, simply drawing the upper fragment down to the lower, and applying nothing to fix or push up the lower fragment.—*Edinburgh Medical Journal, February* 1877, *p.* 708.

52.—ON THE PROPER MODE OF USING CARBOLISED CATGUT LIGATURES.

By R. J. Shepherd, Esq., Resident Medical Officer, St. Olave's Union Infirmary, Lower Road, Rotherhithe.

I read in the Annus Medicus 1876 that "the weight of evidence is strongly against the safety of carbolised catgut ligatures in the treatment of aneurism." As it is possible that the searchers for the reason of this have been on the wrong track, a few practical words on knots may be useful in restoring confidence in what in other respects seems to be universally valued. This is shown by the fact that able men have given it the preference over all the other forms of ligature, which again is shown by their having at least tried it. They would not have used the catgut ligature at all unless they were satisfied that it possessed peculiar advantages, and before it is finally

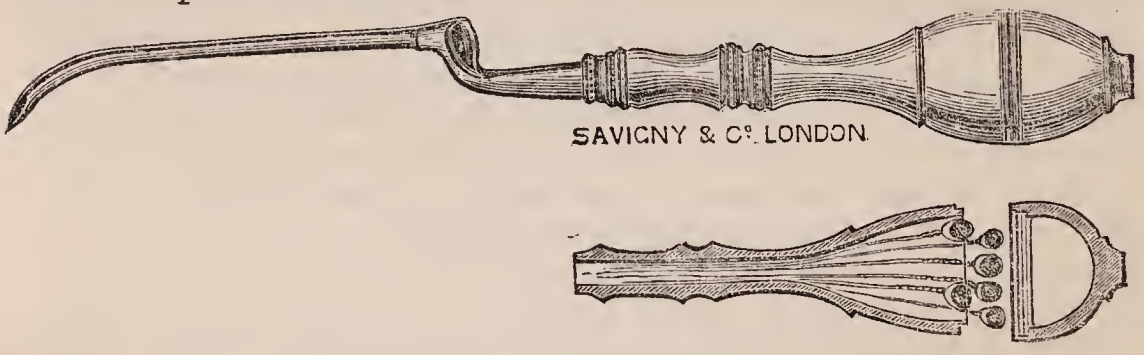
settled by the common consent of those who rule professional opinion that it ought to be discarded in tying arteries in their continuity (no one seems to doubt its value in tying the cut ends of arteries), I, for one should be glad to know if the question of knots has had anything to do with its occasional want of success. I have not used much catgut, but I have tied a deal of fishing-gut (silkworm gut, which I have often thought may be advantageously carbolised in the same way and for the same purpose as catgut), and enough catgut to know that they behave in a precisely similar manner as far as knots go, and nothing slips more easily. As a practical gut-tier I can lay down two principles; the non-observance of either, and *à fortiori* of both, must lead to failure. The knots, in the first place, must be properly formed; and, in the second, must be perfectly made—that is, drawn tight. The first is the most important, as a properly formed knot may draw tight when a strain is put upon it; but absolute safety can only be found in observing both rules. In tying an artery surgeons always aim at making a reef-knot; but as there are four different ways in aiming at this same result, in which a string, with free ends, that has been placed under and across any body, such as an artery, may be tied, and as it seems natural to make the wrong turn, resulting in a granny, which is almost sure to slip, I believe that a simple rule, the observance of which must result in a properly made reef-knot, will be appreciated by many who would perhaps be loth to admit that they found any difficulty, and is worth placing on record. To exemplify: Having placed your ligature under and across the artery, take a free end in each hand; keep the end in the right hand in front of the end in the left hand in crossing the string so as to make it pass across, around and up under the end in the left hand; it will still be in front of the other free end, although it has changed hands; keep it in front in again bringing it across, and complete the knot in the usual way; the result will be a reef-knot. If instead you pass the right hand end (which, after passing across, around, and up under the end in the left hand, is transferred as before to the left hand, and is still in front) behind the end that was in the left hand (but in making the knot has passed to the right hand), in completing the knot you will make a granny. Again, take an end in each hand, the end in the right hand being behind; keep it behind, and pass it, as before, across, around, and up under the end in the left hand, it will come up behind; still keep it behind, and on completing the knot a reef-knot will be the result. If instead, before completing the knot, you allow it to pass in front of the other end, so altering its position with regard to its exit from around and under the other end, you will form a granny. In either case

the knot is begun differently, and may be completed in two ways, a right and a wrong. [The rule simply put is: keep the end that comes up under (from around the other in tying the knot) always the same side.] If it comes up in front of the other end, keep it in front of the other end in completing the knot. If it comes up behind, keep it behind, and you cannot help making a reef-knot; all that remains is to pull it tight. I may add that sailors loosen a reef-knot by pulling one or other of the ends towards the knot. This is invariably done to a certain extent in lifting the two ends for the purpose of cutting them short. The only surgical work that, as far as I know, mentions the reef-knot is Druitt's Surgeon's Vade-Mecum, ninth edition, page 135. Here the knot is figured, but not described, and wrongly called a double reef-knot.—*Lancet, January* 27, 1877, *p.* 151.

53.—NEW NEEDLE FOR CARRYING A DOUBLE WIRE SUTURE.

By Richard Davy, Esq., Surgeon to Westminster Hospital.

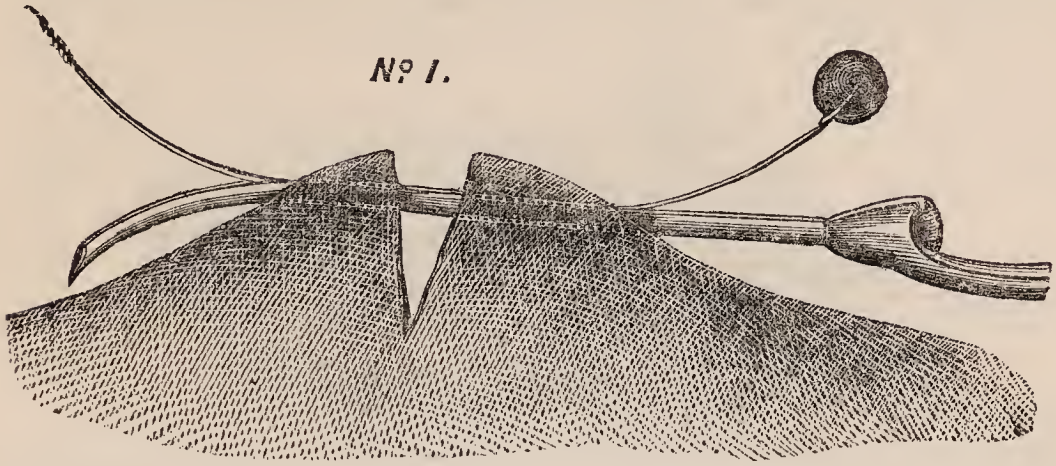
Practical experience for nine years in the use of the tubular needle in operations enables me to speak with confidence of it. My present object is to introduce a modification for the double wire suture, which some surgeons prefer for amputations, hare-lip, deep wounds, perineal operations, &c. I have used the present needle in a variety of operations for the last two years, and am pleased with it. This drawing illustrates the needle



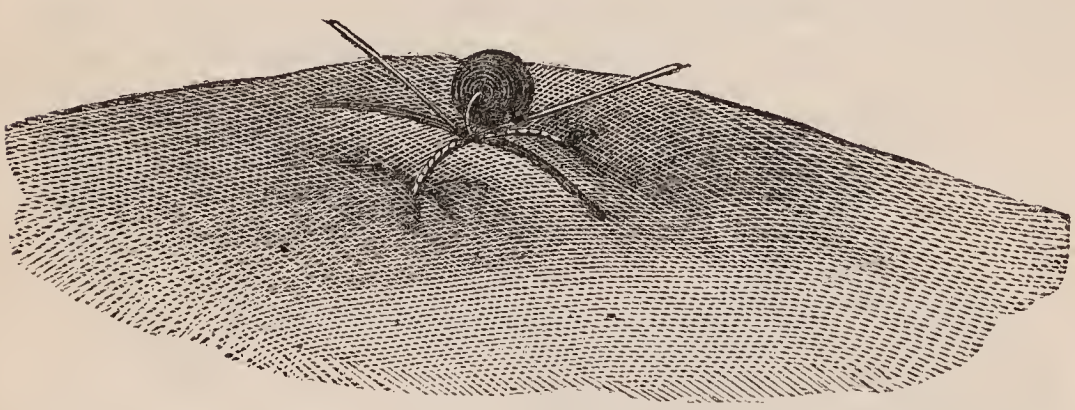
and a section of the handle. The handle consists of a cylindrical box for the beaded wire sutures. The actual needle is slotted throughout its entire length. This slot is necessary for the transit of the bead along the needle, and for perfect washing after use. Complete tubes, especially if small, are difficult to clean thoroughly. The instrument makes an exceedingly good grooved director.

Each wire suture has at one end a black bead (actual size shown below). Through its eye is passed a stout silver or iron wire; a twist of the wires succeeds to the bead; then the two wires are separate and run parallel; lastly, there is another

twist of the wires for a finish. The mode of inserting and fixing these sutures is shown in Nos. 1 and 2. By this



mechanical arrangement, there can be no danger of the suture being buried by reason of subsequent swelling or scabbing; unfolding the wires permits tightening or relaxation, as occasion may require; the eye of the bead admits a tenaculum—by far



the best suture-holder, when the stitch needs removal; and so coadaptation of parts or ablation of wires is easily carried out at the surgeon's discretion. Any variations of securing the wires may be left to the operator's ingenuity.

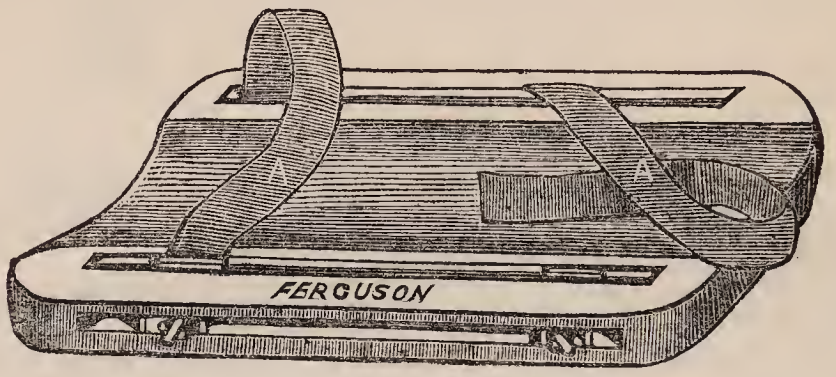
Messrs. Blaise and Co., of 67, St. James's Street, S.W., manufacture the instrument and sutures.—*British Medical Journal, March* 3, 1877, *p.* 270.

54.—NEW SPLINT FOR TREATMENT OF TRANSVERSE FRACTURE OF PATELLA.

By W. E. Steavenson, Esq., Late House-Surgeon at St. Bartholomew's Hospital.

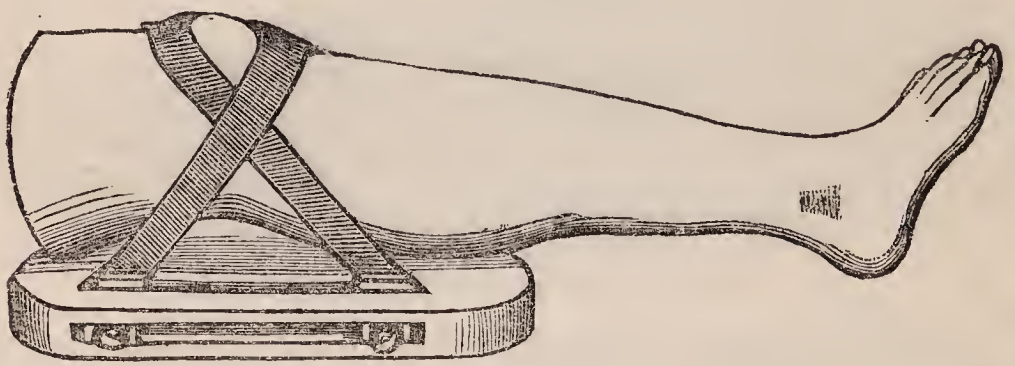
For the treatment of a fractured patella the great difficulty, after setting, is to keep the fragments in apposition without

tilting. To obviate this, in 1874, during my house-surgeoncy at St. Bartholomew's Hospital, I devised the splint represented in the adjoining cut, and used it in several cases to my own satis-



faction and to the benefit of the patient. Had it not been denied by the highest authorities that bony union is possible, I should have considered it had taken place in one or two cases. Since 1874 the splint has been in constant use in Mr. Holden's wards in St. Bartholomew's Hospital, and I am told with satisfactory results. I therefore feel justified in bringing a description of it before the profession.

The splint, as made by Messrs. Fergusson, of Giltspur-street, consists of a short, solid, wooden splint about fifteen inches



long and seven and a half inches wide, rather heavy and thick, and grooved to receive the leg; the upper end of the groove which comes under the thigh is wider than the lower end, which supports the upper part of the calf; there is a vertical and lateral slit on each side of the splint, in which are two travelling brass clamps, worked by screws, for keeping in place the two strips of Esmarch's elastic bandage which, crossed brace-like, keep the fragments of the patella in position. The Esmarch's bandage should be about two inches and a half wide. The great advantage of this mode of treatment consists in this fact. There are many arrangements used in treating fractured patellæ which ensure the apposition of the fragments, but by the width of the Esmarch's bandage we are enabled to place

nearly half of it on each fragment, thereby keeping it from tilting, the greater half of the width of the bandage pulling it against the opposite fragment by the cross arrangement; we thereby ensure a continual and gentle pressure of the two fragments together with their broken surfaces perfectly vertical. In setting the fracture, some soft substance, such as several folds of lint, is generally placed upon the splint to prevent galling, and it seldom requires readjustment.—*Lancet, January* 27, 1877, *p.* 124.

55.—ON THE TREATMENT OF ABSCESSES BY HYPERDISTENSION WITH CARBOLISED WATER.

By George W. Callender, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

We are familiar with the good result which follows the washing out of the sac of an abscess with carbolised water, and afterwards draining it. In some cases, however, abscesses are divided by septa, or have extended amongst tissues so as to form several chambers communicating by narrow passages. They are practically multilocular, and, if washed out in an ordinary way, are not effectually treated, because parts of them are apt to be inefficiently cleansed. In the treatment of all abscesses, but more especially for those with branching sinuses, or with a sac broken up by imperfect septa, we have found it desirable not merely to wash out the abscess in what, for distinction, may be termed the ordinary way, but to throw in such a quantity of fluid as will distend the abscess-sac in all its parts; and this procedure we speak of as hyperdistension of an abscess-cavity. In this manner, abscesses complicated in the way I have mentioned may be cured as effectually as are those in which we have to deal with a single cavity.

The operation may be performed whilst the patient is under the influence of ether, or the integuments may be frozen by the ether-spray. The following are required:—A scalpel where an incision is needed, no open sinus existing; carbolic acid lotion (one part in twenty) diluted to one in thirty by the addition of warm water before using it; a perforated elastic drainage-tube; carbolised oil (one in twelve) on lint for dressing the wound, and gutta-percha tissue for covering this; some ordinary adhesive plaster; some tenax to receive any subsequent discharge (which, however, is very slight); an ordinary two or four-ounce syringe. When it is desirable to make continuous pressure over an abscess after opening it, a pad shaped to the needs of the case, and filled with shot, will be found useful. It acts more effectually than a sand-bag, and is easily made and adapted.

The operation is begun by cutting into the abscess (if no sinus exist), the opening made being of sufficient size to admit one of the fingers. The pus is then allowed to escape, the abscess being emptied as completely as possible. The nozzle of a syringe is next passed through the opening, and the skin is drawn closely around it by the operator with his left hand; the contents of the syringe are then passed into the abscess-sac. Care must be taken in doing this, that no pressure is made upon the abscess-wall, or the distension of the sac will be incomplete. Either by using a syringe which throws a continuous stream, or equally well by closing the wound with a finger whilst the syringe is being refilled by an assistant (very little fluid being lost in its reintroduction), the abscess-sac will presently distend quite to, or even beyond, its original size; and, under these circumstances, the carbolised water necessarily finds its way (as a rule, which has few exceptions) into all parts of the cavity, however irregular, and along any channels leading from it. When the abscess has been opened, the amount of injection may be roughly measured as being rather in excess of the quantity of pus let out. When distension has been effected, the fluid is allowed to escape, and, if much pus be mingled with it, a second injection may be practised. An elastic drainage-tube, its size varying with that of the abscess, is then inserted and secured, and over the end of this, and over the wound, a piece of lint, twice folded and soaked in carbolised oil, is laid. This is covered with a sheet of gutta-percha tissue and some tenax, and these dressings are secured with some ordinary plaster.

Subsequent treatment consists in the renewal of the dressings, which, to myself, it seems desirable to see to daily. The drainage-tube is gradually shortened as the abscess-wall contracts, and through its canal, if there be any sign of puriform discharge, a little carbolised water may be occasionally injected.

It is scarcely necessary for me to add that, under this treatment, the discharge of pus ceases; a limpid serous fluid in small quantity drains away, and presently only a sinus remains; that is, in cases in which there is a persistent source of irritation. These are facts which surgeons have already described.

The point I wish to bring before the Section is, that by hyperdistension of an abscess-sac the carbolised water can be forced into cavities complicated and irregular, and that treatment can thus effect for such complicated abscesses (amongst which may be classed cases of empyema) the same result as an ordinary injection will ensure with a simple abscess.

As for the result of this treatment, so far as bone-caries is concerned, my observations do not at present allow my drawing any absolute conclusions; but that the abscesses connected with

such disease can be emptied and reduced to non-suppurating sinuses, and this without causing the least constitutional disturbance, whilst the health of the patient is improved by the cessation of the suppuration, is clearly established.

I may add that, for these as for other cases, we do not employ the carbolised spray, or adopt any precautions during or after the operation beyond those mentioned, taking care only that the well established rules for surgical treatment are strictly attended to.—*British Medical Journal, Nov.* 4, 1876, *p.* 579.

56.—TREATMENT OF MULTIPLE ABSCESSES BY HYPER-DISTENSION WITH DILUTE CARBOLIC ACID.

By F. Ashton Heath, Esq., Surgeon to the Manchester Royal Infirmary.

Mr. Callender's method of treating large abscess cavities has lately been used in several instances at this hospital with marked success; the following case, which is at present under Mr. Heath's care in the wards, is a remarkably good illustration of its value.

Sarah W., aged 31, admitted December 12th, 1876, married two years, has two healthy children, the youngest not being seven weeks old. She had always had good health up to her last confinement, which was natural in character, and occurred on October 25th, 1876. Four days afterwards, she took cold, and was laid up with pains in the limbs and joints, and profuse sweating, and was said to have rheumatic fever. About a fortnight after these symptoms set in, she had one or two severe shivering fits, and noticed about then a swelling at the upper and inner side of the left thigh, and soon afterwards a second swelling over the right hip. She had continued attacks of vomiting, and was much purged until the time of her admission.

December 12th. She was exceedingly weak, and of a pale cachectic appearance; pulse 124, feeble and irregular; temperature 104·4 degs. At intervals she perspired profusely, and at other times became quite cold. She lay upon her face, and was unable to move her lower extremities. There was every symptom of chronic pyæmia, and at the upper and inner aspect of the left thigh there was a large abscess; a second one occupied the region over the right hip; a third one lay just over the upper part of the sacrum; and a fourth, a smaller one, was found in the left breast. She was ordered two grains of quinine three times a day, good diet, and six ounces of port wine.

December 19th. The largest abscess, that on the inner aspect of the left thigh, was opened with a scalpel; and carbolic acid lotion, 1 in 20, mixed with half its bulk of warm water, was

injected, by means of a Higginson's syringe, continuously into the cavity until it was fully distended, and the fluid returned almost clear. A drainage-tube of perforated India-rubber was then introduced, and the abscess was dressed with "cere-cloth" soaked in glycerine and carbolic acid, and covered with oil-silk. Each day these dressings were renewed, and twice a little carbolic lotion was injected.

After the tapping on the 19th, when nearly eighteen ounces of pus were evacuated, the discharge lost its purulent character, became very scanty, and almost entirely serous in nature.

January 3rd, 1877. The abscess in the back was tapped in a similar manner to the previous one; about seven ounces of pus were removed, and dressings as before applied. The cavity of the other abscess was contracting.

January 6th. The abscess in the right thigh was tapped, and sixteen ounces of pus removed. The cavity was distended as before.

January 8th. The abscess in the breast was tapped, and six ounces of pus removed. The cavity was distended.

January 9th. Temperature 99 degs. The pulse, which the day before had been 110 per minute, was 84. The patient felt better.

January 16th. The patient was on a fair way towards recovery. The wounds in the back, breast, and left thigh were completely healed, and the one over the right hip almost so.

January 21st. The patient was up for the first time. She had a capital appetite, and was rapidly gaining strength.

Remarks.—One remarkable feature in this case is the temperature, which persistently remained at from 100 to 100·4 degs., but dropped to 99 degs. as each successive abscess was tapped, only rising again on the second day. After the last abscess was tapped, however, it fell to 99 degs., and permanently remained at that. Dr. Ross, the pathologist to the hospital, received some of the pus in capillary-tubes as it came from the abscess, and examined it carefully under the microscope, and found it perfectly healthy and free from bacteria.—*British Med. Journal, Feb.* 24, 1877, *p.* 230.

57.—ON THE USE OF SPLIT-TENDON FIBRE FOR SURGICAL LIGATURES.

At a meeting of the Staffordshire Branch of the British Medical Association, Mr. GARNER read a short paper proposing the use of tendon-fibre for surgical ligatures. For some purposes, he thought that even the common silkworm or fisherman's gut might be preferable to catgut. The former is said to be made in Spain from the silk-sacs of the worms, of such as,

from some cause or other, refuse to spin. It is more soluble than silk, in alkaline fluid. Catgut, we are told, is made in Italy from the gut of the sheep, afterwards purified by exposure to burning sulphur, and rounded by being drawn through a hole in a metal thimble. Such as the surgeon uses may be seen, by untwisting after maceration, to be formed of a narrow strip of membrane. It is admitted that catgut has faults; and the principal object of the communication was to show that tendon-fibre is preferable, at least, in some respects and for some purposes. Of all animal substances, tendon (as of the deer or ox) is the strongest, and will, by splitting up, afford ligatures of great strength. It dissolves, but not too soon. A vast number of ligatures may be made by splitting one or two tendons of the long flexors of a ruminant. They may be kept flexible and ready for use in a little spirit and carbolic acid, the plan to be preferred; or, if twisted and dried, they may be made very fine and extremely strong, or they may be simply dried. They may be obtained a foot or more in length. The tie or knot does not slip, and they appear to answer in practice. They swell up like catgut when steeped in alkaline solution, but it is the component cellular tissue only which does so, and it might be possible and interesting to separate it in this manner from the fine glistening and strong tendinous fibres.—*British Medical Journal, Dec.* 23, 1876, *p.* 826.

ORGANS OF CIRCULATION.

58.—TYING THE LEFT SUBCLAVIAN ARTERY IN ITS THIRD PART FOR AXILLARY ANEURISM WITH AN ANTISEPTIC CATGUT LIGATURE.

By Dr. G. Y. Heath.

The records of surgery do not exhibit ligature of the subclavian for axillary aneurism as a very successful proceeding; the cases collected by Scarpa, Travers, Norris, Koch, and Erichsen showing a mortality of about 50 per cent. The snares and pitfalls lying in wait for the hand of the surgeon in this operation are indeed many, and, apparently, there is not one in which it has not at one time or another been caught. It is, in fact, a remarkable thing that every accident that would seem possible has befallen one operator or another in tying this artery. The ligature has been placed round a cervical nerve instead of the artery; the pleura, the vein, and the sac, have each been wounded; part of the vein has been included in the ligature with the artery; the artery has been pierced by the needle; and, lastly, the thoracic duct has been wounded. These casualties, occurring in the hands of the most skilful

operators, indicate that ligature of this artery is often, as Fergusson called it, a troublesome affair, and not to be undertaken inconsiderately. Nevertheless, examples are not wanting in which the result has been a brilliant success: thus, in 1834, Lizars put a flaxen thread round the subclavian, and the wound united over the ligature, cut short, in four days. But even where all has gone smoothly during the operation, the dangers which afterwards beset the patient are such as to make us watch his condition with anxiety. If he escape hemorrhage, pyæmia, cellulitis, or inflammation within the chest, may prove fatal, and even when apparently beyond danger he may succumb from suppuration in the sac.

Impressed with the unfavourable results of this operation and its perilous character, Mr. Holmes, in his exhaustive lectures on the Treatment of Aneurism, states that "the ligature of the subclavian artery is so dangerous an operation that it ought to be restricted to cases where pressure has failed, and to those in which, from the size and rapid growth of the axillary tumour, the surgeon thinks pressure unadvisable." It would seem, therefore, desirable that every case in which this operation has been practised, whatever the result, should be laid before the profession; on this account, and because in my case the antiseptic catgut ligature (which may be said to be still upon its trial) was employed, and also as I believe this is at present the only definitively successful case of the kind so treated, I am desirous of submitting it to the profession through the widely circulated pages of the Lancet. For the details of the case I am indebted to the notes of my dresser, Mr. Craig.

James D., aged forty-eight, cooper, was admitted into the Newcastle-upon-Tyne Infirmary on November 23rd, 1876. He was a big man, tall, broad-shouldered, deep-chested, and muscular. A large pulsating tumour occupied the left armpit, reaching upwards to the collar-bone and downwards to the base of the pit. The pulsation was visible, especially just below the collar-bone and above the upper border of the small pectoral muscle, where less restrained by the muscular wall of the chest. To the hand the pulsation was distinctly aneurismal, and the contents of the sac chiefly fluid. The finger passed readily down upon the subclavian artery on the first rib, but strong pressure on the vessel was needed to stop the beating of the aneurism; and, although the collar-bone was not tilted up, the great muscular development of the man caused the artery to lie at a considerable depth behind the clavicle. There was no œdema of the left hand, but it was colder than the right, and the left pulse was weaker than the right. The left pupil was dilated. The patient complained of aching pain in

the shoulder, back, neck, and arm, and of loss of power and numbness in the fingers. On examination, the heart, lungs, and great vessels within the chest were ascertained to be sound; the sight of the left eye was not materially affected; the urine was free from albumen, and its specific gravity 1012.

History.—Patient knew no cause of his disease. Had not had syphilis, nor recently suffered any accident; was temperate; as a young man had practised pugilism, but after marriage had given that up. In March, 1876, he had felt what he supposed to be rheumatic pains in the shoulder, and experienced unusual trouble in using his two-handed cooper's hammer. In June he first noticed a lump in the armpit; the swelling gradually increased, and by September had nearly filled the hollow. At that time he could not grasp objects so cleverly as before, and the fingers had become numb. He sought advice, and was told to poultice the swelling, supposed to be an abscess. Being subsequently advised to see Dr. Henry Ward, of Blyth, D. consulted him, and was at once told the nature of his disease.

Operation.—On Nov. 28th, five days after the patient's admission, the subclavian artery was tied at its third part with an antiseptic catgut ligature. In doing the operation the perpendicular incision was made short, and divided at once the skin and platysma. No director was used, the deep fascia of the neck being pinched up with forceps, and divided near the base of the omo-hyoid triangle. The external jugular vein was drawn aside by an aneurism needle; no other vein or arterial branch was seen before coming upon the subclavian itself. On raising the fascia the cervical nerves were first discovered; to their inner side lay a yellowish object, not unlike at first sight to the arterial sheath, but which turned out to be the usual lymphatic gland, and some fat. These were removed, and beneath was the artery coming from behind the anterior scalenus. The great vein was seen in front, separated by a distinct space from the artery. At no time during the operation did the vein rise up swollen, and obscure or overlap the artery. The artery was larger than natural, dilated but not distended with blood; it looked and felt flaccid; it could not be seen to pulsate, and to touch its beat was indistinct; its compression instantly stopped the beating of the aneurism. The arterial sheath having been carefully opened, an unarmed aneurismal needle was gently passed under the artery at its lower and inner side, and now it was that the only difficulty in the operation occurred. I have said that the artery was enlarged, but flaccid, not distended with blood. Thus it happened that as the needle was pushed under and round the artery, its flaccid coats were pushed on before the needle. A careless or impatient thrust would have pierced the walls of the vessel; the needle

had therefore to be a little withdrawn, and passed on again. More than once this manœuvre was required, but being fully bent upon passing my needle close round the artery as it lay over the upper edge of the rib, and doing so without either injuring the pleura, unduly lacerating the cellular tissue, disturbing the connexions of the artery, or injuring the last, I persevered patiently, until, by a little humouring, I succeeded in passing my needle as I wished. It was then threaded with a No. 1 carbolised catgut ligature. No force was used in tying the ligature, which was not applied with the object or intention of dividing the internal coats of the vessel. On tightening the catgut pulsation ceased in the aneurism.

But little blood, not more than an ounce, was lost during the operation, which was done according to the antiseptic method. A small drainage-tube was laid from the deeper part of the wound to its outer extremity, which, as the patient was to lie on the left side, would be the most depending point. The usual antiseptic dressings were employed; a smaller (six-ply) gauze cover, large enough to extend some inches beyond the wound in every direction, was placed next the wet padding immediately over the wound, and a larger (eight-ply) gauze cover, such as is used after removal of the breast, was placed over the shoulder, left arm, and side of the chest. A short gauze bandage kept the first cover in place; a longer (ten yards long, four inches wide) gauze bandage kept the larger cover and the arm secure. The larger cover was left off after a few days, when the wound had become superficial; the smaller was continued until cicatrisation was about complete.

The temperature, taken by the nurse in the right armpit immediately after the return of the patient to the ward, was 100°. There was a good deal of pain in the arm at 3 p.m., for which an under-skin injection was made. The patient ate a hearty meal at tea-time, and at 6 p.m. the temperature was 98·6° and the pulse 72.

The after-progress of the case is soon told, for in fact there is little to tell. The patient had simply no symptoms. There was no fever nor pus, but a natural pulse and temperature were noted throughout. Several of the sutures were removed on the fourth day; the remainder, and the drainage-tube, which had already been shortened, on the sixth, when the greater part of the wound was closed, and what remained open was superficial. On the fourteenth day cicatrisation was complete, with the exception of a small point at the drainage-tube opening.

The patient, from the evening of the day of operation, expressed himself as feeling perfectly well, and said that, were it not for his freedom from the old pain in his arm, he should not

have known that anything had been done to him. On November 30th, the second day after the ligature, pulsation was just perceptible at the wrist.

D. left the infirmary about six weeks after the operation. At the time of his outgoing the power was returning in the arm and the numbness had left the fingers. The aneurismal tumour had diminished considerably in size, but was not hard; it gave the impression to the hand of being still chiefly constituted of soft coagulum. I was desirous of retaining him in the house some time longer in order to watch the further changes in the sac, but he felt himself so well that he could not be persuaded to remain, and, in fact, took the opportunity while out on leave of departing entirely.—*Lancet*, *March* 17, 1877, *p.* 384.

59.—ON THROMBOSIS AND EMBOLISM.

By Dr. James Turnbull, Physician to the Liverpool Royal Infirmary.

From an early period, clots and concretions were from time to time observed in the vascular system, which led to speculations similar to those which have occurred in more recent times as to whether they had formed during life or after death. The great German pathologist Virchow was, however, the first to examine the whole subject in a complete and systematic manner, and to put together and explain the isolated facts seen by previous observers without being fully understood in their relation to each other. He placed the whole subject on a firm foundation, and applied the term "thrombosis" to coagulation of blood in the heart or vascular system, and "embolism" to the plugging of vessels which results from the transportation of such clots or thrombi by the current of the blood.

The light thrown on pathology by these investigations was regarded by the late Dr. Parkes as one of the great recent advances in medical science; and, to prove that such is the case, let me point to the embolic pneumonias which arise from plugging of branches of the pulmonary artery, to the sudden deaths which occur in puerperal and other conditions from more complete obstruction of the artery, to the paralytic seizures from plugging of the cerebral arteries, and to the gangrenous attacks in the extremities consequent on heart-affections; all of which were at one time involved in much obscurity.

Systematic treatises on this subject have been published in the German language by Conheim and Cohn, but the researches of British medical men are chiefly to be found in the medical periodicals and in papers in the Transactions of the societies, which abound in cases illustrating the subject, and showing

that the profession in this country is fully alive to its importance. In referring to thrombosis of the veins, Mr. Prescott Hewett, in his address to the Clinical Society in 1873, stated that it is of greatly increasing frequency, but this probably arises from the fact that the true nature of this condition is now more generally recognised.

Before directing your attention to the clots and concretions which form in the vascular system, I would remind you that the coagulation of the blood is produced by the passage of the fibrine from the soluble to the insoluble fibrinated condition. This seems to be dependent on some change in the vital condition of the blood which is not fully understood. Dr. Richardson's theory of its dependence on the escape of ammonia has not been sustained; but the fact remains that the volatile alkali as well as the fixed alkalis have the power of retarding or even preventing coagulation.

In the normal condition, the blood contains from 2 to $3\frac{1}{2}$ parts of fibrin in 1,000, but the proportion is altered in many diseases, and is increased greatly in all the acute inflammatory diseases, especially in pneumonia and acute rheumatism. This increase is the cause of the buffy coat formerly so carefully looked for in reference both to diagnosis and treatment. In the puerperal condition, the blood contains an increased quantity of fibrin, which contracts slowly and presents generally the buffy coat. In chlorosis, it occurs also from there being some absolute increase of the fibrine, together with a large relative increase, owing to the great diminution of red corpuscles. In many cachectic diseases, such as cancer, there is a condition of the blood similar to that of chlorosis, and in all the diseases now referred to the condition of the blood itself predisposes to the formation of clots in the vascular system. In persons of gouty constitution, there is a remarkable tendency to thrombosis in the veins. This may be due to the presence of an acid or other morbid matter in the blood; but Sir James Paget, who has drawn attention to this form of thrombosis, seems to think that there is a local phlebitis. It is, however, worthy of observation that, in the allied disease acute rheumatism, supposed also to be connected with an acid diathesis, there is likewise a tendency to the deposit of fibrin, especially on the valves of the left side of the heart.

With regard to thrombi, they may form in any part of the vascular system. In the heart, we meet with three kinds of clots or thrombi: — 1. The loose, soft, dark-coloured clots, which form ordinarily after death; 2. The firmer yellowish-white thrombi, often adherent to the walls and entangling the valves, which form while death is taking place, or shortly before; 3. The firm, laminated concretions, made up of layers of fibrin with colourless corpuscles, which have existed so long,

that they have often undergone partial softening, especially towards the centre of the concretion.

The second variety of concretions, with which most of us are familiar, form at a variable but short period before death, and they occur chiefly in cases where there has been a severe and protracted struggle, more especially in pulmonary and cardiac diseases. But little attention has been paid to these concretions, and they have scarcely been looked upon as a cause of death, and have been regarded as merely an evidence of how life has terminated. I am, however, disposed to look upon them as in some cases a cause of death which might possibly be warded off if we could prevent their formation, and so gain time for resolution of pneumonia or other diseases with which they are associated. I had lately an opportunity of seeing a *post mortem* examination in a case of acute pneumonia, where the right cavities of the heart were filled with firm whitish concretions, enveloping the valves and cordæ tendineæ, which must have formed a considerable time before death, and where the left ventricle contained also a firm white flattened concretion of still older date. In this case, I think that the immediate cause of death was as much in the blood itself as in the condition of the lung, and that it must have begun with the coagulation of the blood in the cavities of the heart.

In the treatment of this disease, I think we should not overlook the use of any means that can be used to prevent the formation of such fibrinous concretions in the heart. The use of stimulants, which urge on the languid circulation, is clearly indicated; but the alkalis, and more particularly the volatile alkali ammonia, are the only agents we are acquainted with which have a direct power in diminishing the tendency to coagulation, and they should, therefore, be freely administered when we have reason to dread the formation of concretions in the heart. The value of ammonia for this purpose, I may add, was long ago pointed out by Dr. Richardson.

With regard to the class of diseases in which this kind of concretions occur, Dr. Bristowe found that, in a series of forty-one cases, in all but one, which was a case of sloughing sore-throat after scarlet fever, they were cases of either heart-disease, arterial disease, chronic bronchitis, renal disease, tuberculosis, or such a complex of two or more of these affections as we are constantly meeting with in hospital practice.

The third kind of concretions are not always separated by a distinct line of demarcation from the previous one. The concretions, whether in the heart or blood-vessels, which have begun to soften and break down in the centre are often of long standing. I had clear evidence in one case of a concretion in the right ventricle, which was partially softened, of its existence for

more than one month; and, in a case recorded by Lancereaux (Observation cxxii), where several concretions had so softened as to assume the form of fibrinous cysts, the duration was probably much longer. The softening of these concretions produces the puriform appearance often seen in the centre, and the washing away of the detritus is a cause of capillary embolism in various organs: in the lungs, when the thrombus or concretion is in the venous system or right side of the heart; in the liver, when the concretion is in the portal system; and in the brain, spleen, and other organs of the body, when the concretion is in the left side of the heart or larger arteries.

Dr. Ogle reports eight cases of concretions in the cavities of the heart of old standing, some partially converted into a puriform fluid. Of these, five were in the right auricle, one in the right ventricle, and two in the left ventricle.

The thrombi which form in the veins may undergo the same kind of softening, but they also undergo other changes, and may be partially or completely absorbed.

With respect to the causes of thrombi, we have seen that the clotting of blood by which thrombi are formed is caused by an alteration in the state of the fibrin, and the conditions which determine this in the heart or vessels have been fully studied by pathologists, and they may be arranged in two principal groups:—1. Those which produce interruption, stagnation, or slowing of the blood current; 2. Those which produce changes in the vessels or their walls. In many cases, however, these conditions are combined and act together.

To the first group belong the thrombi from ligature of vessels, from the pressure of tumours or abscesses, tubercular or carcinomatous disease, and from surgical injuries. The pressure or stagnation from consolidation of an organ, as in pneumonia or nephritis, may first cause coagulation in the capillary veins, from which it may extend by continued deposition till it reaches the larger vessels. Dilatation, by producing stagnation of blood in the part, causes fibrinous deposits in the veins, when these are in a varicose condition; in the arteries where there is aneurism; and in the heart, where the cavities are dilated.

The long continuance of any chronic disease may, by weakening the power of the heart and the vessels and by lessening the respiratory force which aids the circulation, produce what has been called marasmus or debility thrombosis. In this way, clots form in the sinuses of the brain in chlorosis, and also in other cachectic diseases where the blood is detained by wasting of the cerebral substance. Thus, too, thrombi form in the veins as a consequence of febrile diseases, such as typhus and typhoid, and in tubercular and carcinomatous affections.

The *second* group of conditions which produce clotting—those,

viz., within the vessels, and where the blood comes in contact with an abnormal surface deficient in vitality—comprise phlebitis, endocarditis, endarteritis, and also the roughness of the vascular coats from degenerative changes of fatty or calcareous nature. Also the projection of any foreign body into the vessels or the blood, such as needles, bone, shot, or blood-clots themselves. The passage of pus into the vascular system acts in the same way; and, after fractures, there is sometimes absorption of marrow or fat by the veins, which becomes a source of thrombosis and pulmonary embolism. (On Fat Embolism, see Medical Times and Gazette, January 8th, 1876). Purulent matter may find its way into the veins, not only by the opening of an abscess into them, but also from suppurative phlebitis; the occurrence of which though rare, cannot be questioned. There are thus two kinds of thrombi—those which are caused by mere mechanical stasis, and those which are produced by suppurative gangrenous or other inflammation in or around the walls of the veins. Thrombi of the latter kind, transported by the blood current, carry with them the embolic seeds of disease to the distant part in which they are arrested, and thus we have pyæmic thrombosis and embolism. It is not improbable that pyæmia may sometimes result from the puriform softening of fibrinous concretions formed in the heart or veins; but no doubt it far more frequently arises from those thrombi having access to the external air, and connected with wounds or with the uterus.

Thrombosis and embolism of the portal system and liver have been very fully investigated by Frerichs, who has shown that the same causes which operate in the venous system act here too. Thrombosis and embolism of the vena porta are, perhaps, not unfrequently overlooked for the same reason that they are not observed in other organs; viz., because the organs are more carefully examined than the vessels. But the study of pathology has become a laborious work, and microscopic examination is also necessary to discover the smaller capillary embolisms. If we are, therefore, to advance pathological knowledge, we must continue to uphold our Microscopical Section, and to encourage those who devote their energies to this laborious work.

The third and last branch of our subject comprises thrombosis of the cavities of the left side of the heart, thrombosis of the arteries, and embolism of the branches of the arteries; also the effects of embolism on the viscera and limbs in producing infarction, softening, gangrene, and loss of function.

Concretions form in the cavities of the left side of the heart from conditions similar to those which produce them in the right side, especially in the left auricle from contraction of the

mitral orifice; and Lancereaux describes two cases of concretions in the auricle from mitral contraction consequent on acute rheumatism. He has also reported two cases of fibrinous concretions in the left ventricle. In one, where this cavity was much hypertrophied, there were five concretions, varying from the size of a nut to a pigeon's egg. The larger, adherent to the internal surface, were softened at the centre, and contained a milky looking fluid. In the other, a case of weak, fatty, dilated heart in an intemperate man, there were also several concretions embedded in the fleshy columns, which were softened in the centre.

We know that the left side of the heart is liable to endocardial inflammation, and to valvular affections, which rarely affect the right side; and these conditions cause the formation of smaller fibrinous concretions, which are deposited like granulations on the edges of the valves; and they, as well as the larger concretions resulting from stasis, are a frequent cause of embolic obstructions in the arteries. Rheumatic fever is known to be one of the great causes of endocarditis, and it may produce either valvular contraction or these warty concretions on the edges of the valves. There is, however, another form of endocarditis—the ulcerative—from which embolic matter may be carried away; and this is much more injurious in its effects, being of septic or pyæmic nature; and Lancereaux, who has collected eleven cases, tells us it always proves fatal.

With regard to arterial thrombosis, concretions form less commonly in the arteries than in the veins; but they are produced, as in aneurisms, by dilatation and contraction of the vessels, and also by degeneration of the coats. Last session, Dr. Davidson brought before this Society a case of fibrinous obstruction of the arteries of the brain, which were thickened by syphilitic disease, and this he regarded as thrombosis. Dr. Bristowe (Transactions of the Pathological Society) reports a similar case, where the right hemisphere was softened from obstruction of the middle cerebral artery. He has also reported seven cases of fibrinous deposits in the arteries of the brain, which, he thinks, were not of embolic nature, and must have been due to spontaneous coagulation of blood or local arteritis. In the arteries as well as in the veins, there must often be this difficulty in deciding whether a fibrinous clot has formed in the situation where it is found, or has been transported from a distant part. In either case, however, the effects on the obstructed organ or part will not be materially different.

With respect to arterial embolism, not only the large concretions which form in the left auricle and ventricle, but also the smaller ones which are deposited on the valves, as well as the degenerating matters which escape from both forms of

concretions, are liable to be swept away by the blood current to form embolic plugs in the arterial branches. The clots also which form on the coats of diseased arteries, and likewise the fatty or calcareous matters which form there or on the valves, are carried away and block up the arteries of the spleen, the kidneys, the brain, or the limbs; the relative degree of liability to be affected being in this order, and being due to the particular conditions of the circulating system of these organs.

I need not describe the appearance of infarction and softening presented by each of those organs in consequence of embolic obstruction. We are all, however, I believe, in the present day aware of the fact that embolic obstruction of the arteries of the brain is a frequent cause of softening and paralysis, which cannot be removed by any active depleting and counter-irritant treatment, but is more likely to be benefited by rest and gentle stimulation. And I would remark that, just as Paget, so far back as 1844, drew attention to coagulation of the blood in the pulmonary artery, ably illustrating the subject by a series of carefully observed cases, so also Dr. Kirkes was, in 1852, the first to demonstrate clearly by another series of cases that fibrinous deposits in the interior of the left side of the heart are a frequent cause of softening of the brain, which always results when the obstruction is beyond the circle of Willis, owing to the absence of anastomising branches. In his very original paper, he also illustrated the effects of embolic obstruction in producing infarctions of the spleen and kidneys, and also the violet-coloured petechial spots on the mucous membrane of the stomach and bowels, which are produced by the same cause.—*British Medical Journal, Nov.* 11, 1876, *p.* 611.

ALIMENTARY CANAL.

60.—SOME REMARKS ON THE INTRODUCTION OF THE WHOLE HAND INTO THE RECTUM.

By Dr. W. J. Walsham, Surgeon to the Metropolitan Free Hospital.

These remarks are founded principally on four cases in which during the past year I introduced my whole hand into the rectum for the purpose of diagnosis. Three of these cases occurred in the wards of St. Bartholomew's Hospital, and one in the practice of Dr. Pottle, of Bunhill Row. Although the introduction of the whole hand into the rectum has frequently been resorted to in Germany, the late Professor Simon, of Heidelberg, having especially turned his attention to the subject, it has seldom been practised in this country; the only recorded cases I have come across being those published in the

"Lancet" a few years back by Messrs. Maunder, Heslop, and Batteson. And although I am informed that Mr. Bryant has often adopted this method of examination, I have been unable to find any account of his cases.

As palpation by the hand in the rectum, then, has seldom been practised by British surgeons, it seemed to me that the experience gained in the four cases just referred to, more especially as it confirms Professor Simon's statement that this method of examination is without risk, together with some experiments which I performed on the dead body, were worth recording in the Hospital Reports for the year.

Having been asked by Mr. Smith to examine through the rectum one of his patients supposed to be suffering from stone in the kidney, I thought it advisable before doing so to practise the operation on the dead subject. Ten or twelve experiments were accordingly made.

In *all* these experiments, whether on the male or female subject, the hand, which measures a little less than $7\frac{1}{2}$ inches at its widest part, *could be readily* passed into the rectum. This was a point I was anxious to determine, as Mr. Maunder in his communication to the Lancet of 1868 states that he was unable to introduce his whole hand (the size of which he does not state) into the male rectum, although he had succeeded in doing so in the case of a female; and Professor Simon says that in some of his cases he has had to incise the sphincter before he could overcome the resistance of this muscle.

After the conclusion of the examination a careful dissection was in all cases made, but no rupture of the sphincter or other disturbance of the cellular or vascular connections of the rectum could be discovered; in the majority of cases not even the integumentary fold immediately surrounding the anus was torn, although considerable force was used in many instances.

The hand, when once through the sphincter, could be readily carried up the rectum, and a thorough examination of the pelvic cavity and lower part of the abdomen and their contents made.

In the first two experiments I found little difficulty in passing through the rectum into what, from the distance my hand had penetrated, I imagined to be the sigmoid flexure of the colon; but that the hand really reached this portion of the gut could not be absolutely determined, as the bodies being required for the purpose of dissection, the abdomens were not opened at the time. In the fifth case, the abdomen having been previously opened, I was able to ascertain by actual inspection that my hand had passed into the sigmoid flexure, and by a little manipulation with the other hand I was enabled to put it into the lower part of the descending colon without rupturing the peritoneum.

In the 3rd, 6th, and 9th experiments it felt as if arrested a little above the middle piece of the rectum, and this was found to be the case when the abdomen was opened. The cause of the constriction was then discovered to be a narrowing of the gut where the peritoneum is reflected from the rectum to the walls of the pelvis.

Professor Simon fixes the junction of the first and second pieces of the rectum as the limit beyond which it is unsafe to introduce the whole hand; but in this opinion I only partially agree.

The highest point in the abdomen I was able to reach was the lower end of the kidney and the cartliage of the last rib. When the hand is arrested in the rectum the loose connections of the gut still allow of very free movement, so that even in this case parts above the brim of the pelvis can be examined.

W. P. was admitted into St. Bartholomew's Hospital, January, 1876, under the care of Mr. Smith, supposed to be suffering either from stone in the kidney or from some tuberculous affection of the genito-urinary mucous membrane. His symptoms were—

Painful and very frequent micturition. Urine high coloured, containing abundance of pus and blood on movement. Slight swelling at the lower and back part of either testicle. He was sounded under gas, and the absence of stone in the bladder ascertained. On examination his prostate was found fairly healthy. There was slight swelling and œdema of the left foot and leg. After a few days' rest in bed, the œdema and the blood in the urine disappeared, the latter being again observed when he was allowed to get up and walk about. January 14th, he was examined by the rectum. The patient, who was a big, powerful, and well-nourished man, having been brought deeply under the influence of chloroform, was placed on his right side, with his thighs well drawn up. The left hand being the smaller, was well greased with fresh butter, and the introduction commenced. The sphincter ani was dilated very gradually, first two, then four fingers, and finally the thumb being passed through the anus, and the dilation slowly and steadily continued. On attempting to pass the whole hand, considerable resistance was experienced, but was at length overcome by steady pressure and additional force applied by Mr. Smith to my elbow. The hand now slipped into the rectum, and although moderately grasped by the muscular coat, could explore the pelvic cavity without much trouble. By this means the normal condition of the prostate and the absence of stone in the bladder were confirmed. The parts seeming fairly large, the hand was carried with considerable facility into the sigmoid

flexure, no constriction being experienced in the upper part of the rectum; but there was some little difficulty in following the course of the bowel, as the loose folds of the sigmoid flexure hung about the fingers, and care had to be taken, in finding the passage, not to perforate the gut. On passing the brim of the pelvis, the pulsation of the lower part of the abdominal aorta and of the iliac arteries was plainly felt. The hand being now pushed further up the gut, the fingers could be distinguished through the abdominal walls near the last rib; and on being pressed backwards in the region of the left kidney, nothing that gave the sensation of a stone could be detected.

The operation having lasted full twenty minutes, the hand was withdrawn. The patient experienced no inconvenience from the manipulation. There was no incontinence of fæces, no rupture of the sphincter or integumentary fold round the anus; in fact, the man stated that he would not have known anything had been done to him had it not been for a feeling of slight soreness about the anus for a few hours after the operation. He was discharged from the Hospital.

Whether a stone in the kidney can be detected by the hand in the bowel, is a point, I fear, not settled by this case.

A stone was certainly not felt, and the hand as certainly reached the kidney; but as the patient is still living, it is of course impossible positively to affirm that one was not present.

The second patient into whose rectum the hand was passed was a female under the care of Mr. Holden. She was admitted on January 14th with symptoms of intestinal obstruction of a somewhat doubtful nature. After various remedies had been tried and failed, I was asked to see if I could detect a stricture in the upper part of the rectum. A long tube had been previously passed a foot and a half up the gut, and a pint and a half of fluid injected, the greater part of which had been retained. The patient having been deeply narcotised, the left hand was introduced with the same precautions as in the former case; but on reaching the upper portion of the gut, it was arrested by a tight constriction, through which only the fingers could be passed. As there could be little doubt, from the experience gained in the dead body, that this constriction was due to the rapid narrowing of the gut at the point where the peritoneum is reflected to the pelvic wall, it was deemed imprudent to proceed farther with the investigation, and the hand was accordingly withdrawn. No inconvenience *of any kind* was experienced by the patient from the manipulation. On the 17th, 20th, and 26th the bowels were opened, and average motions passed; but after the 26th they again ceased to act, and the patient gradually sank, and died on the 5th of March. At the *post-mortem*

examination a small epitheliomatous stricture was found in the upper part of the sigmoid flexure. Had the narrowing of the gut not existed, and the parts been as capacious as in Mr. Smith's case, the hand would have probably detected the cause of the obstruction; but as it here failed to do so, the introduction of the hand was possibly productive of harm, as the non-detection of the stricture may have deterred Mr. Holden from performing colotomy—an operation which, as the *post-mortem* shows, would have been successful so far as opening the bowel above the stricture was concerned. But at the same time it must be remembered that this was one of those doubtful cases in which it was next to impossible to come to a correct diagnosis of the situation of the obstruction, for, as before stated, a tube had been passed a foot and a half up the bowel; so, even if the hand had not been introduced, it is not at all certain that any operation would have been undertaken.

The third patient was a female under the care of Dr. Pottle. She also was suffering from internal obstruction of a doubtful nature. Chloroform having been administered, the hand readily passed through the sphincter, and the parts being very capacious, a thorough examination was accomplished. Much fœcal matter was discovered in the upper part of the rectum and sigmoid flexure, the hand easily passing into this portion of the intestine, as the constriction so often referred to was not present in this instance. That the hand really passed into the sigmoid flexure, as above stated, I feel convinced, for the forearm almost as far as the elbow was forced through the anus. Much fœcal matter was removed by the aid of the hand and a long injection-tube, several feet of which were directed by the fingers into the bowel. No bad effects followed this operation, although the hand was withdrawn and reintroduced several times; the sphincter was not ruptured, and there was no incontinence. The patient rallied a few days after the operation, and several motions were passed. She died, however, some eleven days after I saw her, from an attack of severe diarrhœa. A *post-mortem* could not be obtained, but I think I can safely affirm that in this patient no stricture of the rectum or sigmoid flexure existed, and in this opinion I was supported by Dr. Andrew, who also saw her.

I examined the fourth case at the request of Mr. Savory, likewise for the purpose of ascertaining the presence of a stricture; but here, unfortunately, as in case 2, the hand was prevented by the narrowing of the upper part of the rectum from reaching far enough, and no stricture could be discovered. A slight thickening, however, of what seemed to be a neighbouring portion of intestine through the walls of the gut was detected by the hand, though I was unable to give an opinion as

to its nature. No rupture of the sphincter ensued. The patient, who was very much exhausted before the operation, died the same evening, and at the post mortem a small stricture near the commencement of the sigmoid flexure was discovered. Before the parts were disturbed the hand was again introduced into the rectum, and an attempt made to reach the stricture; this was found to be quite impossible, the fingers being fully six inches from the disease when pressed onwards with a force nearly sufficient to rupture the gut. The resistance to the further passage of the hand was clearly seen, as diagnosed during life, to be due to the sudden narrowing of the rectum a little below the sigmoid flexure, where the peritoneum is reflected to the pelvis. On allowing the diseased portion of the sigmoid flexure to sink into the pelvis, it was at once perceived that it was this that had been felt during life as the indistinct thickening through the walls of the rectum; but even with the light of this experience, it does not seem probable that in a similar case such a condition could be diagnosed in the living patient, for it must be remembered that the fingers, when they reach the higher part of the rectum, are held tightly together in the form of a cone by the surrounding bowel, and cannot be separated so as to grasp any structure situated outside the bowel sufficiently well to determine its nature. The only manner in which such a part can be here examined is by pressing upon and carrying the tips of the fingers over it.

Although in the three cases of intestinal obstruction just related, the introduction of the hand was not so successful from a diagnostic point of view as could be wished, a fact of some importance has at any rate been established—viz., that a stricture below the descending colon may exist without the hand being able to detect it. But whether this should deter one from repeating the operation in like cases in future appears at present an open question. On the whole, however, the weight of argument I think tends in the direction of a repetition, for should the hand, the careful introduction of which has been shown to be without risk, detect a stricture, all doubts as to the proper mode of treatment will be removed; but should it fail, no harm need accrue if the surgeon bears in mind that a stricture may still exist though undetected, as in the latter instance he will be no worse off for the introduction of the hand, and can act according to his judgment as if this mode of investigation had never been undertaken.

The whole gist of the matter, however, seems to lie in this—whether in the *majority of cases* in which a stricture really exists the hand will be able to detect it. To settle this point a large number of observations will be necessary, and many experiments on the dead body must be undertaken to determine

accurately the percentage of cases in which the hand can be passed into the sigmoid flexure without impediment from the band-like constriction already referred to. I have already in a considerable number of bodies taken accurate measurements of the rectum and sigmoid flexure at definite distances from the anus, but I am unwilling to make use of these until a still larger number have been accumulated, and shall here content myself with stating that the circumference of the bowel is subject to great variation.

In the three cases of intestinal obstruction, it will be seen that the hand in all failed to detect a stricture; notwithstanding which, in two cases one was discovered after death, and in the third no post-mortem examination was permitted. Setting aside, then, the third case as non-proven—although from the thorough examination obtainable, and the subsequent history of the patient, there seems little doubt to my mind that the absence of a stricture was clearly established—Mr. Holden's and Mr. Savory's cases may be briefly considered. In the former, the stricture being in the upper part of the sigmoid flexure, did not descend into the pelvis, and the narrowing of the upper part of the rectum prevented the hand reaching it, so that by no means could it have been detected with safety. In the latter case, although the hand was unable to reach the disease from the inside of the bowel, still the stricture might have been possibly discovered through the gut-walls; its non-discovery perhaps being due to the inexperience and want of education of the operator's hand, rather than to any fault in the method of investigation.

Palpation by the rectum, however, is probably of more service in the diagnosis of several other diseases of the pelvis and abdomen than in these in which I have as yet had an opportunity of practising it. Professor Simon, indeed, states that he has found it of considerable help in determining the nature of many doubtful affections of the bladder, uterus, and ovaries, and in forming a correct estimate of the extent of the various pelvic deformities. But besides the uses to which this method of examination has been put by Professor Simon, I venture to think that there are several other affections of these regions in which it will be found of considerable service; for, passing my hand over the brim of the pelvis, I was much struck by the facility with which the pulsation in the large branches of the abdominal aorta could be traced to their exit from the pelvis; and it seems to me that in case of aneurism of any of these vessels, a very accurate idea of the extent and conditions could by these means be made out. In gluteal aneurism, also, a careful exploration of the margins of the greater sacro-ischiatic foramen through the walls of the rectum would probably determine

whether or no the intra-pelvic portion of the vessel was encroached upon by the sac, a point, as it is well known, of some importance in deciding on the treatment of this affection. In diagnosing between cancer and aneurism of the pelvic arteries it may also prove useful. Although it is obvious that any conclusion based on the limited experience of this line of investigation as yet obtainable must be necessarily imperfect and liable to modification, still I have ventured to draw up the following, which I think are in accordance with what is at present known of the subject:—

1. That the hand, if small, can be introduced into the rectum of both male and female without fear of rupture of the sphincter or incontinence of fæces.
2. That the dilatation of the sphincter should be very gradual, five minutes at least being allowed for its accomplishment.
3. That no pain or inconvenience is experienced by the patient as an after-result of the operation.
4. That when once through the sphincter, the windings of the gut should be followed very cautiously by a semi-rotatory movement of the hand, and by alternate semi-flexing and extending the fingers.
5. That in many cases the hand can be passed into the sigmoid flexure, and possibly, in rare instances, into the descending colon.
6. That should the hand meet with a feeling of constriction about the junction of the first and second pieces of the rectum, no force on any account should be used to overcome it, as this can only be accomplished by rupturing the peritoneum, which is here reflected from the intestine.
7. That this method of investigation is of use in detecting a stricture high up the rectum or in the sigmoid flexure of the colon, but that a stricture below the descending colon may exist although the hand may be unable to discover it.—*St. Bartholomew's Hospital Reports, vol.* xii., 1876, *p.* 223.

61.—CASE OF INTUSSUSCEPTION SUCCESSFULLY TREATED BY INFLATION: WITH REMARKS.

By Dr. Thomas Eastes, Folkestone.

The following are some notes I took of an interesting case of intussusception recently under my care.

August 6th. I was called at 10.30 a.m., to see W. T., a boy aged 3. I found him lying down, apparently in pain. His mother informed me that he had been quite well until 2.30 a.m., except that for two or three days he had had slight diarrhœa; but this had not been sufficient for her to seek medical advice for him. Between 2 and 3 a.m., he woke up complaining of

great pain in the abdomen; this pain was not constant, but it troubled him again very frequently until my arrival. At 5 a.m., he was sick; there was no food in the vomit, and it consisted chiefly of greenish glairy mucus. At 6 a.m., his father procured some castor-oil, and it was immediately given him. At 6.30 a.m., the bowels acted, and a fairly natural motion was passed. At 7 a.m., or soon after, blood and glairy mucus passed from the bowel with much straining, and twice again before 10.30. The pain was paroxysmal and very severe, making the boy twist and roll himself about all over the couch, and scream to his mother for relief.

On examination, the pulse was a little accelerated; the abdomen was flaccid, and easily examined thoroughly. In the right inguinal region, a firm sausage-shaped swelling was felt, not apparently more than an inch in diameter, and two or three inches long; it was directed upwards, parallel with the side of the abdomen. Three drops of liquor opii sedativus were given to the child; and the mother had strict orders to give him nothing but a tablespoonful of milk every half-hour, and a little ice to suck; and to save everything passed or vomited.

At 1 p.m., the swelling was felt as before in the right inguinal region; but there was now a much firmer and rather larger sausage-shaped mass in the left inguinal region. Its apparent diameter was about two inches, and it could be traced up to the splenic flexure of the colon and partly across towards the median line. It was lost there, and could not be felt again till the swelling previously noticed in the right inguinal region was reached. Each lump was tender, but especially that in the left inguinal region. The symptoms had not improved, except that the child had had some sleep from the opiate, but the pain had been as bad as ever since the sleep, and blood and glairy mucus oozed from the bowel frequently. Three more drops of opiate were given, and only small quantities of milk and ice allowed. At 4 p.m., the parents were told of the very serious nature of the child's illness, and it was explained to them that, if the bowel could not be returned to its proper position, death was almost inevitable; also that I intended, with their consent, to try to remedy the state of things by blowing air into the bowel with a pair of bellows, and, if that did not succeed, to resort to the graver operation of opening the abdomen and pulling the small bowel out of the greater, into which it had been drawn.

At 4.45 p.m., my brother, Mr. George Eastes, who was then staying at Folkestone, kindly met me at the house and administered chloroform to the child. When the child became insensible, I examined the rectum with the forefinger, and could reach well into the commencement of the sigmoid flexure; and

with the other hand could press down the mass on to the tip of my finger, but could not actually touch the end of the invaginated part without two layers of bowel-wall intervening. I now, with the help of the father, proceeded to force air into the rectum. The apparatus used consisted of an ordinary pair of bellows, over the nozzle of which I fastened one end of a yard of India-rubber tubing; the other end of this tubing being slipped over the end of a gum-elastic tube, such as is usually sold with Higginson's syringes. This gum-elastic tube was then passed into the rectum. It had at its base an ivory plate of the size of a halfpenny, which helped to retain the air in the rectum. After blowing the bellows for about four minutes, the sigmoid flexure and descending colon became inflated, but there was no great distention for a quarter of an hour; this was chiefly due to imperfect action of the bellows, which I then attended to, and was able to improve temporarily; but there was also what one might call a safety-valve action of the rectum, so that, when the distension of that part of the bowel was too great, some of the air escaped by the side of the tube, and blood and mucus were forced out with it. The distension now became more visible; and after about half an hour, it was evident that the air had passed through the intussuscepted part and through the ileo-cæcal valve, and that the small bowel was partly distended. The process of inflation took about forty minutes altogether. From time to time, the action of the bellows was stopped, and the abdomen manipulated gently to help the return of the bowel to its natural state; but the air was never all allowed to escape, until the end of the operation, when it was desirable to make as perfect an examination of the abdomen as possible before allowing the child to recover consciousness. Within ten minutes of commencing the inflation, the mass had retreated upwards from the left inguinal region, and soon it could not be felt. When the abdomen was quite flaccid again and the child still insensible, there was a slight feeling of resistance in the right inguinal region, probably due to the swollen state of the extremely congested part of the bowel that had formed the deeper and more advanced part of the intussusception; and I had strong hopes that we had accomplished the desired object, and saved the child's life without running the risk of opening the abdomen.

At 9 p.m., the child was sitting up in bed, comfortable and laughing, and would not admit that the abdomen was tender, but I was careful not to press very heavily. The pulse was 122 per minute; there had been sickness once, and an attack of slight pain once since the operation, and a considerable quantity of blood and mucus had been passed.

August 7th, 11.30 a.m. The child had been very comfortable

indeed; his pulse was 92; there had been no further action of the bowels, but a little vomiting on two occasions, the vomit consisting of some of the milk and ice-water that he had taken, and having a slightly green tinge. He had had one very slight pain in the night. The abdomen was not so flaccid, but not at all tender, and no lump or swelling could be felt on either side. His diet still consisted of milk and ice.

8.30 p.m. The pulse was 100, and the boy was comfortable; there had been no further sickness; no tenderness nor swelling was discoverable. The bowels had acted naturally at 8 p.m.; no blood nor mucus being passed. There had been no more pain; and from this time the boy continued to improve rapidly, having no other bad symptoms.

Remarks.—Cause. I believe that the increased and irregular peristalsis accompanying diarrhœa is a common cause of intussusception; and this is in agreement with the fact that intussusception is much more common in the summer months than in winter.

Diagnosis.—The case could not be mistaken after a careful examination of the abdomen, and the history alone made one strongly suspect its true nature; the mother's account of slight diarrhœa for a few days, then sudden severe colicky pain and vomiting recurring at intervals, followed in rather over four hours by the passage of blood and mucus, with much tenesmus, in a child under six years, made the existence of intussusception highly probable; and the presence of the tumours rendered it quite certain. It seems to have been a case of the ileo-cæcal variety of intussusception, such as is much the most frequent in children. There is a little uncertainty as to the real character of the tumour in the right inguinal region. When first I saw the child, it was probably part of the bowel involved that was felt there; and, having once felt it unmistakably, I, perhaps, did not afterwards pay so much attention to it as to the mass in the left side of the abdomen. For, when the intussusception was plainly felt in the left inguinal region, the tumour in the right could only have been the portions of meso-colon and mesentery corresponding to the intussuscepted bowel stretched extremely tight.

Prognosis.—This, apart from the chance of successful treatment, was very bad. Recovery of children by separation of the invaginated part is a rare result. Under treatment, the prognosis was good, because the intussusception was not probably of more than fourteen hours' duration when inflation was practised, and, therefore, so much the more likely to be reduced by that process.

Treatment.—I think there can be no doubt that, when once intussusception is diagnosed, an anæsthetic should be given,

and inflation tried as the most successful treatment known; and, if that be entirely unsuccessful, the tumour not changing its position at all, I believe that, where the diagnosis is certain, the proper step to take is to open the abdomen at once, and draw out the bowel from its enclosing sheath of neighbouring bowel as carefully as possible, and close the wound. This, of course, applies especially to cases seen early; for there is no doubt that immediate treatment is of the greatest importance. By delay, the intussusception will probably increase; if not, adhesions may form, or strangulation of the enclosed bowel proceed to a dangerous degree, both complications lessening the chance of successful treatment so much that, after awhile, it is wiser to trust to the small chance of recovery by gangrene and separation of the intussuscepted portion of bowel than to attempt to return it to its normal position. In thirteen cases (referred to in the New Sydenham Society's Biennial Retrospect of Medicine and Surgery for 1873-74, page 341), in which the abdomen was opened for intussusception, five recovered. This is an encouraging result; and, I believe, far above the number that recover after the separation of the bowel.—*British Medical Journal, Dec.* 30, 1876, *p.* 850.

62.—A CASE OF OBSTRUCTION OF THE BOWEL, SUCCESSFULLY TREATED BY A SYPHON-ENEMA, FOLLOWED BY HYPODERMIC INJECTION OF MORPHIA.

By Dr. J. Pearson Irvine, B.A., Assistant Physician to Charing Cross Hospital.

This case would seem to be of interest both clinically and therapeutically: therapeutically, because it demonstrates the value of certain methods of treatment which, so far as I can ascertain, have not been at all tried in this country; clinically, for various reasons which will appear in the narrative.

Jane B., a widow, aged sixty-eight, was admitted into Charing-Cross Hospital on Friday, July 23, 1875, under the care of my friend Dr. Silver, of whose beds I at the time had charge. She was a spare, slightly-built woman, who had borne several children, and who, up to the change of life, had enjoyed unusually good health. At this period she began to suffer from headache, and from various trifling disorders of digestion, which increased so that during the last four years she had experienced repeated attacks of constipation, supervening on a generally sluggish condition of bowel. She varied a good deal in her dates from time to time when questioned, but it was certain that so long as four years ago she had an attack of costiveness lasting many days, and that during the last twelve months such attacks had become more frequent and more

aggravated as regards duration and general symptoms, one having lasted a month, and having been attended by severe abdominal pain and repeated vomitings. She was admitted for constipation, and stated that she had passed nothing by the bowel since June 28, at which date she had, for the first time for a month, a slight evacuation; so that, practically, at the time of admission there had been obstruction for seven weeks. There was no history of cancer in the family, and between the attacks she had always been perfectly well, not losing flesh to any marked degree. She had never vomited or passed blood. There was no history of syphilis. In short, no history could be obtained of any disease likely to be followed by stricture of the intestines.

On admission she was comparatively free from pain, but complained of occasional seizures, during which the "stomach was drawn up." The face was a little anxious; the brow more deeply furrowed than age would account for. She was dull if not disturbed, and volunteered but little information. There was no headache; the mind was clear; the eyes bright; and the complexion good for one of her years. The tongue, coated white, was not dry. Occasional vomiting occurred after food. Thus the general symptoms were comparatively slight, but the local condition serious. The abdomen was greatly enlarged, and, at times especially, very irregular in shape; the umbilicus was obliterated; both flanks looked full; everywhere was a feeling of semi-solid resistance (especially marked about the umbilicus), but nowhere could a distinct tumour be made out. There was well-marked tympanitic note in the flanks, but a "patchy" muffling of the note anteriorly, mostly about the navel. The edge of the liver could not be felt; its dulness ascended superficially as high as the fourth space in the nipple line. Spasm of the intestine (afterwards observed as a regularly recurring symptom) came on during examination, and immediately the shape of the abdomen was altered. Its surface, especially in the epigastric and umbilical regions, became irregular; a large tumour-like mass came into view, occupying these parts, lobulated in look and to touch, and to the latter tense and firm, but lost both to eye and hand gradually towards the hypochondriac regions, though prolonged more distinctly to the left than to the right. The tumour was pear-shaped, and more like the stomach in this respect than anything else, but its narrow extremity was three inches distinctly to right of the middle line, and it was undoubtedly an immensely distended transverse colon. Its lower edge was sinuous, and crossed the abdomen about two inches below the navel. During "spasm" the contraction of small intestine was equally well defined through the retracted abdominal wall, declaring itself by a wave-like rip-

pling between pubis and umbilicus as the peristaltic action apparently began, and proceeded, to terminate at its acme in the transverse colon. These attacks were repeated at short intervals, lasting about half a minute, and passed off gradually; they were accompanied by pain, which the patient likened to "grinding labour pains." The abdomen, as might be expected, was at such times dull on percussion; but in periods of rest percussion could not define any particular tumour, and the appearances were those of a large and pendulous abdomen, as if indicative of a lax state of the viscera. Thus, the flanks became fuller when the patient turned on this side or that, but irregularly and not in the manner met with in ascites. There was no true tenderness of the abdomen; but manipulation excited spasm-pain, which the patient confounded with tenderness. On admission the temperature was normal—respirations 30, pulse 90. Patient was put on a diet of beef-tea and milk, with small doses of brandy.

From July 23 to 26, enemata, sometimes of gruel and castor oil, sometimes containing oil of rue or turpentine, were assiduously used by Mr. Henwood, the Resident Medical Officer, without any good result. They were returned almost immediately, unchanged. Thirty grains of sulphate of magnesia were given every four hours. On July 27 a long enema-tube was passed without difficulty, and twenty ounces of gruel, with three ounces of castor oil, were thrown up. This injection was returned in five minutes unchanged. There was no trace of fæcal matter on the tube. A tight flannel roller gave temporary relief from the spasmodic pains. Ten minims of liquor potassæ in tea were prescribed to be taken twice a day.

On July 28 there was no change. Two minims of croton oil were given the preceding night, without the least result; patient did not vomit. Enemata had been used four times a day, but on this morning the long tube could not be passed more than about three inches. Finger in the rectum could detect nothing beyond a very lax condition of mucous membrane; the uterus was natural in size and position. Half a grain of opium in pill was ordered three times a day.

On July 29 the patient was worse. She had vomited frequently during the last twenty-four hours, especially after food. Spasms were severe, the intestines rolling under the hand with every attack. No motions; and the long tube had been passed with the utmost difficulty. Opium was continued through this and the following day, so that on

July 31 the pupils were contracted, and the abdominal pain much diminished. There had been no evacuation, and vomiting persisted. The tongue was dry and foul; the countenance very anxious; pulse, respiration, and temperature unchanged. It

was found impossible to pass the rectal tube more than two or three inches, and thus far only with much pain. On this day, assisted by my friend and colleague Dr. Green (to whose advice I was much indebted) and by Mr. Rodwell, the Assistant Medical Officer, I had fitted up over the patient's bed, at a height of nearly three yards, a pail of about two gallons' capacity. This was filled with lukewarm water, and by means of an elastic tube, three-quarters of an inch in diameter, attached to an ordinary long rectal tube, the contents of the pail were, by syphon-action, injected in a continuous stream into the bowel. The rectal tube, prior to commencement of injection, could be introduced scarcely a quarter of its length, and to this extent only with difficulty, but immediately the flow of water was permitted, and dilated the intestine before it, the tube passed with the greatest ease. It was therefore clear that the lax condition of the mucous membrane or the collapse of the walls of the intestines prevented its progress, rather than any permanent stricture. The continuous flow of water caused, of course, some annoyance, and towards the end distress. The water returned as quickly as injected, but by means of firm pressure with a cloth around the anus we were enabled to retain it at intervals for short periods, at the same time preventing too forcible mechanical action likely to result by slightly pressing the syphon-tube, and thus moderating the current. The operation lasted thirty minutes, and about two ounces of scabrous masses were discharged, these being distinctly fæcal. It was unlikely that any, or at least that much, of the injection was retained, as was expected; but the operation was in a sense immediately successful, insomuch as no trace of fæces had appeared in any previous injection. Slight signs of shock were quickly recovered from. Patient was ordered to continue opium and sulphate of magnesia, but to have no enemata until seen next day.

She passed a bad night, and next morning (August 1) asked, for the first time, for something to relieve her pain. There was no tenderness over the abdomen, and no headache. However, the propriety of colotomy was talked of. But a dull, irregular tumour could be seen and felt in the region of the umbilicus, more distinctly to the left than to the right. The abdomen looked larger. No spasms came on during manipulation, nor at other times, as before. It was resolved to wait, and to continue the opiate alone.

On August 2 an enema-tube could not be passed more than two or three inches. The abdomen was now especially tense in the left hypochondrium. Spasm had recurred, and during its occurrence was evident a decided change in the shape of the colon. Just above the umbilicus—a part previously very prominent during spasm—existed deep hollowing. Dulness was

decided in the left hypochondrium. Pulse 90, steady and firm; temperature 98·5°. Patient complained of great pain, and lay constantly on the left side. She had vomited repeatedly, but nothing stercoraceous. The legs pitted on pressure. One-sixth of a grain of morphia hypodermically was ordered three times a day, and enemata were continued in the ordinary way. Patient, at her own request, had a warm bath, which gave very marked temporary relief from pain.

On August 3 an enema of gruel and castor oil returned after ten minutes, mixed with liquid and scybalous fæcal matter. There was now marked dulness along the course of the descending colon. Next day fæcal matter was again passed with enemata of gruel and oil, one of which was retained three hours. There was more decided dulness, with doughy feel in the left iliac region; but the shape of the abdomen was much more uniform, and the coils of intestine did not stand out in the same way as before. Pain was, however, still severe at times. Morphia was stopped, and liquor atropiæ was ordered to be injected subcutaneously in three minim doses twice a day. Next day August 5, fæcal matters passed in larger quantities; a free flow of urine followed the use of the atropine (an enema had been retained some time), and the pulse rose to 96. During this day there began a continuous "running" of liquid fæces, and the abdomen became lax, so that it was easy to feel lumpy masses in the descending colon.

On August 6, liquor atropiæ being continued, an enema brought away a vast quantity of fæcal matters. The pupils were widely dilated. No vomiting; tongue cleaner and more moist; pulse 100. On August 7 larger quantities were passed: the rectum was found to be loaded, and a catheter was insinuated through its contents as high as possible, and enemata given through it, with the best results. The shape of the abdomen was now completely changed; the largely distended tranverse colon had disappeared, but it was still easy during spasm (now more regular and less distressing) to make out the intestines between the umbilicus and pubis. Considerable pain was still complained of, but there was no vomiting, and the patient took plenty of concentrated beef-tea with sherry. Large evacuations were constantly passed, and on August 9 the abdomen was quite small, and its walls so relaxed that a roller was applied for their support. Up to August 16 atropine was continued in the manner and doses mentioned, and during the time of its use there was a greatly increased flow of urine. Strychnine was then substituted, the abdomen at the time being everywhere flaccid, and the bowels acting daily without enemata. The obstruction had indeed, to all appearances, been (whatever its nature) completely overcome.

Note.—The patient whose case is narrated above subsequently died of obstruction, and I had an opportunity of seeing the post-mortem examination. About an inch to two inches above the left end of the transverse colon was an annular stricture, caused by fibroid thickening of uncertain origin, not completely occluding the intestine, but barely admitting the passage of the little finger. Immediately above the stricture was a recent ulceration of the coats of the intestine. The whole of the large bowel above was immensely dilated, the dilatation being greatest near the stricture, so that the transverse colon had indeed the shape of the stomach, with the larger end projecting downwards into the left lumbar region. The walls of the dilated portions were thickened. The intestines above the stricture, as high as the ileo-cæcal valve, were filled with uniformly solid fæcal matters (weighing seven pounds), which looked as if packed and pressed into them. In the ileum, for a considerable extent upwards, were quantities of liquid fæces, but this portion of bowel was not distended like the parts below, though considerably dilated. The transverse colon, like a huge solid roll, occupied a great part of the abdominal cavity. The descending colon was collapsed, as were also the sigmoid flexure and the rectum; their mucous membranes were lying in relaxed folds. These portions of intestine, before being opened, had more the appearance of arteries than of bowel. These post-mortem conditions bear out, perhaps, most of what has been said as to the possibilities of an accurate diagnosis of the seat and nature of the obstruction, and certainly testify as to the value of the treatment adopted. Probably during the subsequent attack, of which the patient died, the stricture had become more and more impermeable, ulcerations, consequent on previous attacks, having led by their after-healing to this condition. Perhaps, also, repeated seizures, and consequently repeated dilatations above the seat of stricture, had finally induced such an imperfection in intestines that they could no longer contract with sufficient regularity and power to discharge their fæcal contents, no matter what the remedies employed to assist them might have been.—*Medical Times and Gazette, Nov.* 25, 1876, *p.* 592.

63.—ON THE APPLICATION OF TRUSSES TO HERNIÆ.

By John Wood, Esq., F.R.S., Professor of Surgery at King's College, and Surgeon to King's College Hospital.

I propose to speak of the application of trusses to hernia. It is one of great practical importance. I shall commence with inguinal hernia, which is by far the most common; and we will consider what you may expect to accomplish by trusses in the

way of cure, the kind of truss to be applied, and the sort of apparatus which is either of no use or positively injurious, and is therefore to be avoided. Femoral hernia comes next in point of frequency in the adult; and lastly we will consider briefly the truss treatment of umbilical hernia.

The relative positions of the apertures which are concerned in inguinal hernia are to be carefully recognised. Under the term oblique inguinal herniæ are included congenital and infantile, because children's herniæ descend along the course of the spermatic cord. In the infantile variety the tunica vaginalis is imperfectly closed, and extends high up the canal, and the rupture-sac comes down within it, so that you have reduplication of the serous sac of the rupture by the addition of the front layer of this serous tissue. Now let us study the hernial apertures. First of all, placed a little inside the midway point between the anterior superior iliac spine and the spine of the pubis, you have an internal or deep ring which transmits the constituents of the cord. This is not a direct opening, which you can see on the superficial as well as on the deep surface, but it is like the opening of the sleeve of your coat in relation to the body, which is seen inside and not outside. If you look at it from the outside you will see the infundibular process of fascia passing from the transversalis, covering the opening by being prolonged over the cord, and forming its intimate investment. In the opening of the coat-sleeve the edge is most prominent on the inner part, where the armpit of the wearer is often galled by it. Now it is just so in hernia. That is the point also where the internal abdominal ring galls the protruded bowel, and sometimes strangulates it. We will leave out of consideration altogether the relation of the epigastric and other vessels, which scarcely bear upon the treatment by truss-pressure. The inguinal canal slopes downward and inward under the lower borders of the internal oblique and transversalis muscles; and when the hernia has dilated this canal and arrived at the external ring, it forms an opening placed above and outside the cord. Then the hernia, if permitted to go on, slips under the coverings of the cord, and becomes scrotal hernia. Generally speaking, there is a little constriction in the upper part of a scrotal hernia, indicating the commencement of the unstriped muscle, or dartos, of the scrotum, and the termination of the fat of the tegumentary tissues. This sometimes gives a complete hour-glass appearance to the sac, such as is not uncommonly also seen in hydrocele.

There are various degrees of oblique inguinal hernia. First of all, when the bowel begins to pass through the internal ring, it does not usually come through suddenly. In very few cases does oblique inguinal hernia occur suddenly at this point of its

progress. It is prepared slowly, till at length, on the patient making an effort, it advances a little further, so as to give him pain, and draw attention to it. It occurs in this wise:—There you will see is a ring, and behind the ring is the peritoneum. The peritoneum there is loose, because it has to provide for the superior false ligament of the bladder, and to allow of the distension of that organ. If the sub-peritoneal fascia and the fascia transversalis are feeble and their support to the peritoneum insufficient, the latter gets so thinned and weakened as to yield before a cough or any lifting effort. Even the slightest causes constantly repeated will slowly and often unobservedly produce a protrusion. At length the hernia gets into the upper part of the inguinal canal, when it is called a *bubonocele*, *i.e.*, a hernial tumour which is still within the inguinal canal, or just emerging from the superficial ring. Even in this condition it may get strangulated at the neck of the sac as it passes the internal ring, the inguinal canal having become dilated, sometimes to very much greater extent than the internal ring itself, which may still remain at little more than its normal size. You frequently find in these cases a large cavity which the hernia has made for itself in the canal between the layers of the abdominal aponeuroses, with no external evidence except a fulness in the groin and a pain or sense of weakness, increased on coughing or other exertion. At length the pillars of the external ring give way, sometimes by a quick, sometimes by a very slow process; their edges are first bulged outward; the external pillar carrying with it the cord, and the internal pillar being relaxed and curved inwards. This process is effected by the yielding of the arciform bands of the external spermatic or inter-columnar fascia, which gradually allows the pillars to become bulged forwards, and then everted and curvilinear instead of nearly straight in direction.

Now let us consider how we should best restrain or prevent this process. It must be done by judicious truss pressure, by which even a radical cure may be obtained in some cases. To accomplish this, however, requires so accurate a fit, so appropriate an instrument, and such care on the part of the patient or, in the case of children, on the part of the nurse, that it is not often successful. About 15 to 20 per cent. of hernia patients may be cured by judicious and persistent truss pressure, perhaps more in the case of children and young persons, and certainly less in adults. The reason why in the herniæ of children we are more likely to get a cure by truss pressure is this:—In children, the condition is the result, both in the case of inguinal and umbilical hernia, of an imperfection in evolution shown in the final closing up of the abdominal wall. The former is generally caused by a late descent of the testicle,

accompanied, probably, by an imperfect development of the cremaster and gubernaculum testis. The whole of the parts concerned in the descent of the testis are weakened and backward in development; the consequences do not always show themselves immediately after birth, but may become apparent at some later period of life. Not uncommonly a portion of the peritoneum is drawn down, preceding the testicle itself in its descent into the scrotum. Sometimes the testicle is held in the abdomen by adhesions, while the epididymis is stretched out and unravelled, reaching from the testicle, which remains below the kidney where it is developed, as far as the external ring.

This fact of the peritoneum preceding the testicle gives rise to a peculiar form of rupture, which you sometimes see in children, viz., the testicle is retained in the abdomen, while a hydrocele or serous effusion forms in the tunica vaginalis, producing a tumour which simulates a bubonocele; a portion of bowel or omentum may now slip beyond the testicle and pass into the sac. The bowel may come down with the fluid in the tunica vaginalis, leaving the testicle behind. These are sometimes called "windy ruptures," and the fluid contained therein may be pressed back into the abdominal cavity. The descent of the testis takes place about the end of the seventh month of intra-uterine life, and ought to be completed before the end of the eighth; but it varies very much according to the forwardness of the development of the child. The gland ought to be in the scrotum at the time of birth, and it is the duty of the doctor to ascertain whether it is so or not at the birth of a male child. It is generally some weeks afterwards before the canal is closed up. The closure begins at the upper part, and makes its way downwards until it seals up the tunica vaginalis about half an inch from the testicle. A similar process, producing a protrusion of the peritoneum into the groin, may occur in a female child, the ovary passing into the labium instead of the pelvis, and sinking further into it as the pelvis develops. Sometimes, by an error in development, a process of peritoneum is drawn down into the labium, and this should close up as in the male; but sometimes it remains patulous, and gives rise to a congenital inguinal hernia in the female. In such a case as that you should apply pressure early, directly backward at the site of the deep or internal ring.

We will now consider the mechanical action of *the truss*. There are two parts of the truss for separate consideration. The chief and most important part is the pad. All trusses for reducible hernia are provided with some sort of pad, whatever may be their principle of mechanical action. The object of this pad is to press upon the openings through which the rupture passes, to keep the bowel or omentum from getting into the

canal, and, if possible, to prevent it from slipping down into the scrotum, even if it passes the internal or deep abdominal ring. There are various shapes of pads, some of which are very objectionable in principle. I take one here for example. This is a very conical pad, so conical as to be almost bluntly pointed. Put this on a weak place, and what ensues. There is a hole beneath the integuments, which are spread over that hole, covering it in. The conical or acute truss-pad presses these superficial tissues into the hole, in much the same way as when you put a cork into a bottle with a piece of leather over it. It thus spreads the tissues out, stretches and weakens them, and, at the same time, dilates the tendinous aperture of the superficial ring. The injurious effect of wearing continuously trusses of this kind, or their various modifications, all of which have the same radical vice, is increased in some by a powerful spiral spring placed inside the conical pad, so that they press the tissues still more powerfully into the hernial opening. The movements of a patient who wears such a truss cause a constant working of this spring, and a boring motion into the aperture is produced, like the twisting of a cork into the neck of a bottle. Moreover, in a case of rupture you have, not a resisting bottle-neck, but an elastic and valvular opening which yields to the pressure. You are continually obliged, therefore, to increase the size of your cork-like pad, so as to fully occupy the hole and sustain the rupture. The aperture regularly increases, and the rupture, when it does come down, constantly becomes larger and more liable to become scrotal, until at length it gets so large and unmanageable that no truss will keep it up. This is a common result of wearing a truss-pad of this kind.

It has been asserted that as a fact the projecting conical pad only buries itself in the subcutaneous tissues, and does not project between the pillars of the ring. But this seems a purely arbitrary assumption, and one certainly contrary to the results of observation in cases which have been subject to such pressure for a length of time. If there be no pressure exerted by the point of the cone, what effect can it have in restraining the rupture at all? If it has any effect at all upon the inguinal canal, that effect must be in accordance with the shape of the impinging surface. A conical wedge-like pressure cannot be transformed into a flat pressure by skin and fat, which are almost as yielding as water. And what occurs in the numerous cases in which the skin is thin and delicate, and the fat almost entirely absent? A rounded surface must press on all sides in a direction perpendicular to the surface which presses. It must tend, therefore, to thrust outwards the pillars of the ring, and to stretch and weaken the inter-columnar fascia in the same way as the introduction of a Wutzer's plug into the canal.

And that it does so I have verified by post-mortem examination in numerous cases where a truss had been worn. In fact, a conical surface fitting into a slippery and elastic opening, rather favours the escape of the hernia between its sloping sides and the edges of the aperture, as soon as the rupture acquires sufficient power to lift up the truss-pad a little; and if it becomes displaced laterally the hernia immediately slips down the inclined plane. Sometimes in such a case the instrument-maker, finding the rupture to slip down into the canal, and wishing to stop it in its descent, prolongs his truss-pad downwards into a sort of tail, and makes it bigger and bigger, until at length he pushes aside the scrotum, and may bring the pad down into the perineum. Such arrangements hardly ever do any good at all; they are simply excuses for "fiddle-faddling." They are exceedingly uncomfortable to the patient, and you may be sure if a rupture gets down the canal so far as to need such a secondary pad, it is certain to pass on into the scrotum; you cannot stop it. The most important indication, therefore, is to prevent the rupture from entering the canal at all; to shut up the internal ring altogether.

The pads are fastened on to the retaining apparatus in a variety of ways. Some are made so as to be adjusting, with the idea of following the rupture in the various twistings of its course and emergence. After long experience, I have come to the conclusion that nothing useful can be done by such pads as these, unless the patient is constantly on the watch to adapt the pad to the shiftings of the rupture. This, I need not remark, can scarcely be done in society, or in the street, or in the various situations where the stress of a rupture may come. The best way is to have your pad so fixed that it prevents the rupture from getting into the canal at all. The kind of pad I myself recommend—what I chiefly insist upon—is, first, that the bearing of the surface of the pad shall be *flat*, that it shall not press in the tissues or invaginate them into the canal between the pillars of the external abdominal ring, and thus stretch, fray, and weaken the inter-columnar fascia which ought to restrain the rupture from coming down. The edge, of course, must be rounded off to prevent it cutting. We get, therefore, to this kind of flat-bottomed-boat shape according in outline with that of the inguinal canal in its diseased condition: that shape is an oblique oval. In oblique inguinal hernia in the female a flat oval pad, without any break in its outline, answers very well indeed. In the male, however, there is a peculiarity in the anatomical arrangement of the parts. The spermatic cord passes out of the superficial abdominal opening external to the spine of the pubis, crossing or lying over the outer pillar of the ring. If the truss-pad produces a pressure upon the cord, it

not only makes the patient uncomfortable and the pad more liable to shift and slip about, but also may cause swelling of the testicle, hydrocele, varicocele, and ultimately atrophy; while the chafing of the pad against the pubis leads to the formation of excoriations, sores, and even abscesses. In order to avoid this, we have in the truss now before us a chink or slit in the pad (see fig. 1). This gives the pad a sort of oblique horse-shoe shape. If properly put on, the shorter end lies upon Poupart's ligament immediately outside and above the spine of the pubis; the longer end lies on the inner pillar, and the round end covers the deep hernial opening of the external ring. The cord, being very movable, will adjust itself to the pad and slip into the part where there is the least pressure, *i.e.*, into the chink left between the two points of the pad.

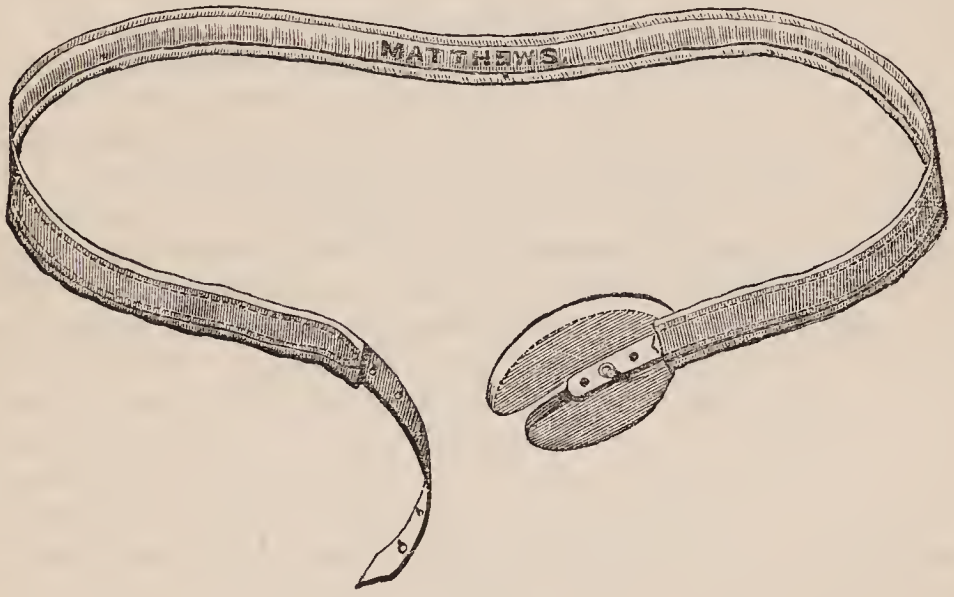


Fig. 1.

If you hope to get the inguinal canal closed up, and the sac obliterated by a radical cure, it is better to have the pressure hard and firm; and the best material for the pad is a substance called "vulcanite," of which a specimen is before you. This does not absorb the perspiration, is perfectly smooth and hard, and, if proper care is taken to keep the surface clean and dry, will not chafe or give rise to sores. The next best substances are box-wood and ivory, which, however, absorb the perspiration to a somewhat greater extent. Experience of these trusses in hot climates has been unanimously in favour of the hard vulcanite over any other substance for truss pads. Leathern or parchment coverings become putrid, foul, and hard, under the effect of constant absorption of the cutaneous excretions, and get so nasty that sensitive and cleanly patients cannot bear to wear them. In other cases, where you do not go in for a radical

cure so much as for making a patient comfortable, then water and air pads, made up of india-rubber upon a metal frame, are exceedingly comfortable and useful. Some patients cannot bear any other than this soft pressure. They are of the same general shape and principle, but the surface is more yielding, and the pressure is soft; they cannot press into the hernial apertures as hard conical pads do, and the pressure being fluid is equal in all directions.

The nature of the apparatus for fixing and keeping on the pad and restraining the rupture is also of great importance. The rupture requires pressure to retain it, and, as a rule, you do no good at all unless there is a side-spring. There have been various ways devised of applying this retaining apparatus. One way which the patients sometimes choose, and which seems to recommend itself to them by its simplicity, is having a strap round the body, and an understrap across the perineum. Now it is exceedingly difficult to wear a band round the waist so tight as not to give way to pressure at one point, and so to yield before the rupture. Even if you could make it tight enough the patient could not wear it, the tightness would be so great. You may take it, as a rule, that these straps round the pelvis, when a patient is exerting himself and contracting his abdominal muscles, are of no use in keeping in a rupture. Where there is real need for pressure, nearly all truss-makers have recourse to some form of the side-spring. Some have the spring passing only round one half of the body, with a pad behind, on the sacrum. This pad is always flat or oval, and slightly concave, and is larger and thinner than that placed on the rupture. It is held in its place by a strap that goes round the opposite side of the body, and frequently by an understrap across the perineum. Some have the spring put on the same side as the rupture; but in Salmon and Ody's truss the spring is put on the opposite side, so that it reaches across the front of the abdomen, and is longer than those which are put on the same side as the rupture. The rupture pad projects more than the posterior one, and works upon a ball and socket-joint. The spring is longer than is necessary to go only half round the body; it reaches over to the opposite side, and the support it gives depends upon the fact of its pushing upwards and towards the ruptured side. Other half-round springs depend upon the power they possess of pressing or pulling upwards and outwards towards the same side, in opposition to the descent of the rupture. Sometimes it is necessary to wear a perineal band which buttons in front. Generally speaking this may be dispensed with after the truss has accommodated itself to the shape of the body, which, after a time, all trusses do to some extent. The warmth and motion of the body will make even the spring accommodate itself somewhat to the shape of the body.

With regard to the bend of the spring there are one or two matters of very great importance. In the first place, the spring should go round the body at a level midway between the projection of the trochanter and the anterior superior iliac spine. There it lies on the tensor vaginæ femoris and gluteal muscles, and does not work over bony surfaces. That is the level at which the measurement for a strap should be taken when you have to send for a truss to a maker. But I may here remark that it is never satisfactory to send measurements without the maker seeing the patients. It is as if you were to send measurements to a tailor or a shoemaker to make your trousers and shoes from. You would scarcely be likely to have a good fit. The maker wants, in addition, to comprehend the shape of the back and set on of the pelvis as well as the mere dimensions. So that, if possible, you ought to bring the maker and patient together. Other plans do not usually succeed. The spring should point down far enough to get to the opening, and the pad should be placed upon the opening. You will see that the spring requires to be bent down a good deal more for crural than for inguinal hernia. For crural hernia the side-spring should be made like the handle of an old-fashioned pistol. That end of the spring which bears the pad should project well, so as to give a proper degree of backward pressure; and if you look at the surface of the spring you will see that it is somewhat twisted on its own axis, so as to give an outward and upward pressure as well as a backward pressure. This gives the right direction in which to keep the hernia in the abdomen when it tends to pass into the canal. The round part of the horse-shoe pad presses upon the internal ring, and the ends press upon the pillar of the external ring. The chink is to lodge the cord, which is thus held as if embraced by the fingers employed in reducing and keeping in the rupture. Thus the rupture is prevented from coming through the internal ring, while the pillars of the superficial opening are prevented from separating, and so allowing the rupture to pass out. The length of the spring from the point where it comes round the hips should be duly proportioned to the patient's formation. In these horse-shoe pads there are holes and screws by which the pad can be shifted a little to adjust this properly. If the spring be too long at this part the pad presses against the outer edge of the rectus muscle. The inner border of the pad should be parallel to the outer border of this muscle, and the outer border should lie upon Poupart's ligament. If the spring be too long it pushes the pad further on to the muscle, which bears off the pressure from the hernial cause during the contraction of the muscle, permitting the rupture to escape below and outside the pad. If, on the other hand, you have the spring too short the rupture will escape between the rectus and the pad.

A direct inguinal hernia passes through the triangle of Hesselbach, enclosed between the epigastric artery, the edge of the rectus, and Poupart's ligament. That is the area you have to protect; and it can best be done by a flat-rounded or oblately-oval pad, fitting close between the edges of the rectus and Poupart's ligament, reaching well down to the crest of the pubis, and provided with a slight notch below for the passage of the cord. To keep the pad from shifting upwards and from pressing unduly upon the pubis, care is required in adjusting the action of the side-spring. It is as well to wear at first an under or perineal strap, until the pad and spring have adjusted themselves to the shape of the abdomen. In corpulent persons a considerable upward slope may be also given to the surface of the pad to make it lie parallel with the slope of the abdomen, and to prevent the upper edge from pressing unduly into the flesh. In thin persons, with lean flanks, the tendency is always for the pad to slide upwards into the hollow formed by the abdomen. This can be met sometimes by keeping the surface of the pad quite flat, so as to lie perfectly level upon the surface of the groin. You may, however, in the course of time, in the same patient find a marked alteration in the slope of the abdomen from an increase in the abdominal volume, as well as in the thickness of the superficial fat, altering entirely the conditions of the rupture and the requirements of the pad and spring. In some instances this may occur in a very short time. On the other hand, a patient from illness or active work may get rapidly thin, and require a re-adjustment from this cause. To meet and manage these conditions is one of the niceties of truss-making. It is sometimes difficult to get the exact twist, and even when you have got it right the condition of the patient may change, and you may have to alter the spring accordingly.

The problem to solve may be put geometrically: it is requisite to obtain the angle of inclination of the abdomen to a transverse vertical plane, taken at the most prominent part of the inguinal region, and containing the side of a right-angled triangle, of which the posterior wall of the inguinal canal is the hypothenuse, and the horizontal level of the upper margin of the pubis is the base. If you do not have the pad-surface inclined enough the rupture comes down under its lower border, and if you twist it too much you get the same edge pressing in so as to inconvenience the patient, and allow the rupture to enter the upper part of the canal. It is this slipping over and under on one side or the other that constitutes the troublesome part of the treatment of ruptures by trusses. You do not often find patients who have sufficient mechanical knowledge or who take sufficient pains to aid the efforts of the instrument-maker by skilful adjustment of the pad after a careful return of the rup-

ture. This is one cause why so few cures are effected in this way. I recommend, as a rule, the all-round spring covering over both hips instead of the one-sided spring; but in certain cases I believe the principle followed in Salmon and Ody's plan, a half-round spring fitted to the opposite side of the hips, and pushing towards the ruptured part, may be advantageous; the horse-shoe form of pad, however, may be used quite as well with this form of spring.

In old cases of irreducible hernia you meet with another difficulty—you cannot reduce the hernia entirely, and all you can do is to prevent more of the intestines from coming down. In such cases the bowel may be exposed to all sorts of injury, besides constituting a deformity of a somewhat conspicuous character. To remedy this you must have a suspensory or bag truss made of stout jean, or some unyielding material, which will keep a constant pressure upon the contents. If the irreducible portion consist of omentum only, you must also have some pressure over the inguinal canal to prevent the bowel from following the omentum. Such combinations are sometimes exceedingly difficult to carry out. Messrs. Matthews have tried, in some of my cases of this kind, with much success, a truss-pad shaped to the form of the rupture, composed of a frame of stout wire, well padded, and stretching between the wire framework a bag of stout jean, or of some slightly-elastic material, sufficiently resisting, into which the hernia is received. The wire framework, pressing all round the irreducible rupture, keeps it well in hand and under control. All you can do in such cases is simply to make your patient as comfortable as circumstances will allow, and to prevent injury to the irreducible rupture.

In *crural hernia* we have conditions entirely different. The inner opening is constituted by the crural ring, a horizontal aperture with a slight inclination forward. In front it is bounded by Poupart's ligament, on the inner side by Gimbernat's ligament, and on the outer side by the femoral vein and artery. These are structures which vary somewhat in tension. Relaxation of the muscles of the abdomen has a great effect upon Poupart's ligament. But the greater part of the surrounding structures are composed of unyielding ligamentous tissue. so that there is not that contraction and relaxation that is present in inguinal hernia. A little below there is another opening, called the saphenous opening, directed forwards and a little inwards, and almost vertically, but with a slight inclination downward. Lying in front of the passage between these two openings is the upper part of the process of Burns or femoral ligament (Hey's), which extends from half to three-quarters of an inch downwards from Poupart's ligament, with

which it is continuous above, to the margin of the saphenous opening.

It is this part to which the pressure of a truss should be applied in crural rupture, when it will protect both the crural ring or upper, and the saphenous opening or lower, aperture of the crural canal. Immediately outside the canal are the femoral vein and artery, which must not be pressed on by the truss, and below is the saphena vein, which it is also important not to compress.

When a femoral rupture gets fairly through the saphenous opening, it turns upward and outward round the edge of the falciform process, and lies over the femoral vessels and upon Poupart's ligament. In order effectively to deal with this rupture, you must altogether prevent it coming through the crural ring into the canal before it makes the upward and outward turn, so as to lie upon Poupart's ligament. If you fail in this, then your truss pressing the rupture against the falciform process of Burns thereby injures the bowel, and does harm rather than good, and the patient would be safer and better without a truss at all.

The truss-pad for crural hernia must protect the crural ring by pressure over Poupart's ligament, and it must also press upon and fill the saphenous opening. It must not press downward, so as to obstruct the saphenous vein. The pad will be apt to slip, so as to miss the crural canal altogether, and, by irritating the inguinal glands, may cause trouble. The best form of truss-pad for this hernia is the one I show you (see fig. 2). The outline is an egg-shape, with the small end down-

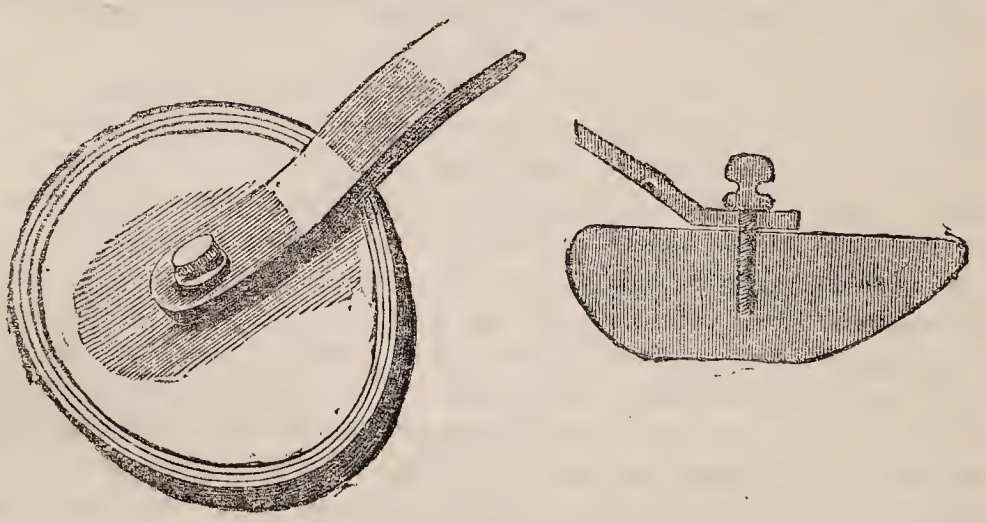


Fig. 2.

wards: it is adapted to the shape of the saphenous opening, but rather longer, so as to press upon Poupart's ligament with its broad end above. The side spring is fixed exactly in the centre. If you look at the section, you will see it slopes off

below, so as to avoid pressing upon the saphenous vein, and forms a rounded projection above, so as to fall into the fold of the groin upon Poupart's ligament when the patient sits down. It is thus adapted for keeping in position; for the truss-pad which most adapts itself to the form of the surface will stop in its place the best.

In the truss for femoral hernia, the pad end of the spring is bent downwards in a large curve to permit the patient's thigh to bend freely and without obstruction in sitting. You ought not to be content with seeing your patient stand when you fit on a truss; you must make him sit down on a low seat, and then stand, walk about, and jump from a stool, and see if that dislodges the truss. If the truss does not hurt him, but keeps the hernia up, under those conditions, you may conclude it will do for all the ordinary purposes of life. The commencement of a radical cure by truss pressure always dates from the last time the bowel or omentum came into the sac of a rupture. Hence the importance of the patient preventing the hernia from ever coming down. If it come down even once, he has to begin *de novo* from that point to produce the obliteration of the canal. Hence, a patient who wishes to get rid, at the earliest possible period, of a disagreeable and troublesome deformity must wear his truss on all occasions, night and day; he must never assume the erect posture without it; and if he bathes, he must have a bathing truss, for sometimes in the gymnastic movements which generally attend upon a cold bath the rupture may come down.

One or two words with regard to *umbilical rupture*, and its apparatus. Umbilical rupture is exceedingly common in children, and in them it is usually curable. It comes through a natural opening which is left for the umbilical vessels up to the time of birth, and which it is the tendency of nature to close up sooner or later. That tendency is very strong, and the only thing that prevents it is the bowel constantly coming into the sac. If you can, in a child, manage to prevent this, you cure the hernia; and that is generally the case when the improved apparatus of the present day is carefully attended to. But there are some cases where the child is not tractable, and from pain and fretfulness is often crying and screaming; then you get the rupture distended violently and constantly. Again, if the nurse is not soothing and careful you seldom get the rupture cured. The ordinary rough-and-ready and often very successful fashion of treating umbilical hernia is covering a flat piece of metal, say, one of the bronze coins of the realm, a penny-piece, with plaster, with the sticky side outwards, putting it on the projection, and strapping it across the abdomen with broad straps of adhesive plaster.

In some of the older books on this subject you will find recommended a convex cork plugging up the aperture, like the neck of a bottle; but elastic apertures of vital tissue cannot be blocked up in that way, while the cork tends to make matters worse by dilating the aperture, and thus keeping open the rupture. Therefore that is one of the things to be avoided. It does not keep in the rupture, because it will slip out at the side of the cork. A flat surface, rather larger than the aperture, is what you ought to have. A flat penny-piece, or bit of lead of the same shape and size, may be backed up by a thicker piece of wood or cork, and the strapping may be put across. In this way a very good and easy apparatus is made, if the patient cannot afford to have a proper apparatus; but it involves the necessity of a tedious process of taking off sticking plaster, which is sometimes not done in the gentlest way, and thus sets up a crying bout, and brings the bowel out of the aperture. All this is inconvenient. When you are called upon to do this, you must press the parietes of the abdomen together with your finger and thumb, so as to close the umbilical hole before you take off the pad and strapping, and take care the bowel does not slip out.

A very capital invention is one produced by Messrs. Matthews, a very ingenious adaptation of elastic india-rubber, arranged in two compartments, distended with air, and communicating by a small aperture; a central one, globular in shape, and an outer ring. The former presses upon the umbilical opening, and the outer upon neighbouring tissues forming its boundaries, and so prevents the umbilical hernia from coming out under a cough or cry impulse. This central portion acts like the penny-piece, with the additional advantage of becoming tightly distended by the air from the surrounding ring cushion, forced through the small aperture of communication by the impulse of the abdominal muscles. The whole is held on by an elastic band round the body, and can be distended after fixing by blowing through a little stop-tap. By this means the moment the bowel has a tendency to escape through the hernial aperture it is met and forced back again by the dilating globe. By this means we have produced some very capital cures in children. In adults also, in whom it is much more difficult to produce a radical cure, this apparatus is very useful and comfortable. The chief reason why in adults you do not get a radical cure of umbilical hernia is because it is generally accompanied by abdominal obesity and laxity, the stomach too becoming at intervals much distended with food and flatulence, and the mesenteries being enlarged by an accumulation of fat. In such persons you must be content, even in any kind of hernia, with amelioration of their condition rather than cure. But in young persons you

may frequently succeed in effecting a cure by the aid of a proper instrument. If, in young persons, a hernia of the inguinal or umbilical variety resists the cure by careful mechanical restraint, then it becomes a question whether you cannot safely and greatly increase the chance of a cure by an operation, which keeps out the bowel for a sufficient length of time for the opening to contract and close.—*Medical Examiner*, *Feb.* 8, 1877, *p.* 104.

64.—NOTE ON THE IMMEDIATE CURE OF PILES.

By H. A. Reeves, Esq., Assistant-Surgeon at the London Hospital, and Surgeon to the Hospital for Women.

During the latter part of last year I commenced the treatment of piles about to be described, and having now submitted eighteen cases to this new method, and sufficient time having elapsed to form a fair judgment as to the result of most of the cases, it is time to make the simple operation more public, so that others may try it, and report their results. All the patients operated on suffered from the severer form of internal piles, and four of them were bad cases—*i.e.*, the piles were very large and ulcerated in large superficial patches, and the general condition was distressing, as there were anæmia and haggard aspect due to hemorrhage and pain.

To this rapid method of treatment I have applied a term used by Mr. Barnard Holt, and now so well known to the profession—viz., the *immediate* cure; and I have used the word *cure* advisedly, as the first batch of patients have not had the slightest trouble since they were operated on. The last five cases are too recent to say anything as to ultimate results.

I feel that the term "immediate" is more strongly applicable to this method than to that of Mr. Holt, for not only is the operation rapid, but the *entire treatment* is very short as compared with the ordinary methods of treating hemorrhoids—*i.e.*, by nitric acid, ligature, or clamp and cautery.

In the *immediate treatment* of urethral stricture—excellent as I have come to regard it in fit cases—the operation is speedy, and in the majority of cases safe; but the *treatment* is very prolonged—nay, life-long; and the *cure*—well, never. In the *immediate cure* of piles, I can truly state that, so far as my present experience goes—and this can be corroborated by several witnesses and by the patients also—the operation is rapid and trifling, and may in some cases be done without anæsthetics; it is unattended with the least risk, and the *cure* permanent.

The operation is simply this. The piles being well down, they are punctured with the conical pointed end, which I have

had made by Messrs. Mayer and Meltzer to fit on to Dr. Paquelin's gas cautery (and which is figured in Mr. Joseph's paper in this volume of the *Retrospect*), to their bases, the number of these hot punctures varying with the number and size of the piles, a pile the size of half a small walnut requiring two or three. A dull-red heat should be used, and the point gently rotated while being extracted, and *not pulled out*, because if this be done a portion of the eschar will be withdrawn with the instrument, and some hemorrhage will follow. Should the disease be of old date, some of the piles will be quite hard; these I have pierced to their softer attachment, at the feeding veins of which they were clot-laminated, and even fibrous varicose transformations. Ulcers and fissures in connexion with the hemorrhoids were touched with the cautery.

If this simple plan be properly followed, there is no hemorrhage, but should there be slight oozing, a touch of the cautery at once stops it; the piles are then returned, and a half-grain morphia suppository introduced. The bowels are kept confined by a quarter of a grain of morphia daily, by mouth or subcutaneously, for the first two or three days, and on the fourth or fifth day an enema-tube is gently introduced and a warm injection given, and followed on the succeeding day by a laxative. The first two, or in some few cases three, motions, produce pain, but nothing as compared with that the patients suffered *before* the operation; and at the expiration of a week they are discharged, with such directions as to diet and regimen that will promote the healthy functions of the rectum, and which are known to all professional men.

It is right to state that two of these eighteen cases were not allowed out for ten days, and one for a fortnight, but in all there was some other pre-existing complication, either urinary or uterine. Sixteen of them were treated at the Hospital for Women, and two in private. I have seen them all several times since, and examined them with finger and speculum, and I can say that the satisfaction of the patients at their rapid and permanent relief is not greater than was mine when I observed how little damage was done to the rectum, as evidenced by the difficulty of detecting, some little time after, any result, in the shape of cicatrices, of the operation. The ages of the patients varied from twenty-three to sixty.

I am happy to say that I have not yet had an opportunity of examining *post-mortem* any case operated on, but I conceive that the *rationale* of the method is that the igni-puncture sets up a phlebitis which soon leads to obliteration of the diseased veins; that the phlebitic clot is, somewhat rapidly, sufficiently absorbed, or so altered as to render it difficult for the finger to detect any nodules or lines of thickening in the rectum. What-

ever the traumatic pathological change may be, certain it is that the *symptomatic* relief is not only speedy but lasting. I may mention that I had occasion to operate on a patient for urethral mischief, who had undergone this procedure for piles three weeks previously, and neither I nor others present could discover the least trace of any recent operation on the rectum.

I do not wish it to be thought that I consider the operative results, as regards nodulation and disappearance of the altered piles, will always be so rapid; this may or may not be so without affecting that which the patient and the surgeon most desire —viz., the cure of the case. I briefly sum up what I consider the advantages of this method over the old plans.

1st. The operation is quickly done.

2nd. The cure is much more speedy, as, by the ligature or clamp and cautery, three weeks is considered quick time for convalescence.

3rd. There is no fear of secondary hemorrhage, as there is no ligature to separate, and no wounded surface to cauterise.

4th. Nothing is removed. To the patient this is very often a strong recommendation; to the surgeon, at first and without experience of this method, it may seem a drawback, but sufficient trial will convince him to the contrary.

5th. There is no apprehension of secondary abscesses and fistulæ so far as my experience has gone.

6th. There cannot possibly be a stricture as a result of the operation. That this has occurred several times after the old methods no one can gainsay, and I may quote a case sent me by Dr. Heywood Smith, on which I operated by the clamp and cautery, and only removed the piles and not a particle of other rectal tissue, and in seven weeks had to commence the use of bougie for an annular stricture near the orifice. Nothing of the kind pre-existed.

7th. There are no relapses. Two of the cases I operated on had been elsewhere treated by ligature, and the other with clamp and cautery. Of course, if all the diseased part be not punctured at the time of operation, the portion left untouched may be the source of future trouble, necessitating an operation, and it may be that this was the explanation of the relapses in the two cases just mentioned. On the other hand, it is fair to state that other veins, already weak at the time of operation, but not sufficiently so to attract attention, subsequently enlarged and required meddling with.

8th. In patients who can bear a little pain no anæsthetics are necessary, as the operation is a quick one.

It is obvious that this plan can be applied to other varicose veins and to nævi.

Before concluding I may mention that I have, in two cases,

tried the revived plan of sudden dilatation of the sphincters; one did moderately well, the other had to be igni-punctured. I have, in one case, injected the piles with solution of perchloride of iron undiluted, but the result was not satisfactory. I believe, however, that a weaker injection of iron, or of water and iodine, or of chloral, would be effectual, and have the advantage of not needing anæsthetics.—*Lancet, February* 17, 1877, *p.* 229.

ORGANS OF URINE AND GENERATION.

65.—SIR H. THOMPSON ON STRICTURE OF THE URETHRA.

(From a Review by the Editor of the British and Foreign Medico-Chirurgical Review).

During the last few years, both on the Continent and in America, there has arisen a tendency to reduce the treatment of stricture as much as possible to a merely mechanical proceeding, in which, after a guide has once been obtained to the bladder, an ingeniously contrived machine may be trusted to complete the cure. This mode of treatment, perhaps, reaches its highest development in the hands of Dr. Otis, of New York. According to him, a certain relation exists between the circumference of the penis and the calibre of the urethra; and having ascertained the former, he cuts the urethra up to the size he considers the right one, taking no consideration of the varying diameter of the normal urethra at its various parts. The urethrotome cuts everything alike that resists it, whether it be a pathological stricture or a normal narrowing of the canal. In the same way the system of forcible dilatation advocated by Holt splits or stretches the urethra equally through its whole extent. In defence of these mechanical modes of treatment it is alleged that, from the soft and yielding nature of the structure of the normal urethra, only such parts as are abnormally indurated or narrowed can suffer, while the healthy textures escape, and there is no doubt that this is more or less true of forcible dilatation. The same cannot, however, be said of internal urethrotomy, as performed by Dr. Otis. If it be attempted to cut a urethra up to one uniform size, and that the maximum diameter of its healthy parts, it is evident that many other parts that are not really strictured must suffer in the operation. Against these purely mechanical modes of treatment Sir Henry Thompson protests most strongly. He points out with great force that, as in every other disease, the mildest means of treatment consistent with efficiency should always be employed, and that to cut or forcibly rupture every stricture indiscriminately would be as wrong and as unscientific as it

would be to amputate every diseased knee-joint without first trying what may be done by milder means. In the treatment of stricture, therefore, the surgeon should never, if he can help it, go beyond simple dilatation. This, Sir Henry Thompson asserts, acts merely mechanically, by "stretching the organized lymph, which forms bands round the canal at the strictured point." He denies that it in any way produces absorption of this tissue, as is usually supposed; yet it seems to us that it must be within the experience of most surgeons that, after one passage of a catheter, a larger instrument will often pass in a few days without any appreciable resistance. Moreover, if the process is purely mechanical, why not stretch the urethra up to the full size at once, instead of spending days or perhaps weeks in the gradual process of dilatation? In this mode of treatment Sir Henry Thompson always prefers soft to solid instruments, and his opinion is the more valuable as he tells us himself that he has completely changed his opinions since he first wrote on this subject. In the later stages, however, he strongly recommends the employment of polished conical steel bougies, varying about two numbers of the English scale from the point to the thickest part. These views will no doubt meet with the approval of almost every practical surgeon at the present time; and doubtless a vast amount of human misery would be saved if the French bulbous bougies and catheters formed a part of every surgeon's stock of instruments. At the present time it is still too common, especially in the country, to have recourse in every case to solid instruments.

If simple dilatation fail, the next resource is continuous dilatation by tying a soft catheter into the urethra. There is no doubt this treatment is marvellously rapid and usually perfectly innocuous. A urethra which at first only admitted a No. $\frac{1}{2}$ may in a few days take a No. 12 with ease. The troubles usually met with arise from the cystitis that is so often set up by the presence of the catheter in the bladder. Moreover, the catheter opens up the bladder, as it were, to the air, and so favours decomposition of the urine, which infallibly aggravates any existing cystitis. Now, cystitis is not a thing to be lightly set up, as it is not always easily allayed again, and the tendency to its occurrence has always been considered a drawback to this mode of treatment. In order to prevent it Sir Henry Thompson recommends the catheter should be tied in, in such a way that the eye is only just inside the bladder, and be accurately fixed in this position by being fastened to the penis itself and cut off to the proper length. If this simple precaution be taken, Sir Henry Thompson states that cystitis seldom gives rise to any trouble, and after eight to ten days simple may be substituted for continuous dilatation. But excellent as these

modes of treatment doubtless are, they frequently must fail on account of the resilience of the stricture, never on account of mere tightness. All strictures in front of the scrotum and many in the bulb show this character of resilience in a marked degree. Now, in these cases Sir Henry Thompson's next resource is internal urethrotomy, and for this operation he prefers Civiale's urethrotome, cutting from behind forwards, to any other, except of course, for the orifice or first inch, in which parts a small bistori caché serves every purpose. Before attempting internal urethrotomy he would always dilate the urethra by continuous dilatation to about No. 6, a size which just admits of the passage of the urethrotome. He prefers this mode of operating to any more purely mechanical means, because he believes it can be intelligently directed by the surgeon. On this subject he writes as follows:

"Now, suppose for an instant that it were possible for you to have the stricture before you open to your eyes and hands; you certainly would take a little scalpel and employ it for each case according to its needs, intelligently dividing the obstructing fibres as much as is necessary, and no more. But with very little practice it is perfectly easy to do this without seeing the stricture—that is, to apply a little scalpel, dividing where you wish, and not elsewhere, just as you divide the constricting fibres in the ring of a hernial sac. And I need scarcely say this intelligent action of yours will produce something better than the work of a machine, which any ignorant person can employ just as well as you. The difference in the two cases resembles that which exists between the music of an organ produced by a handle and the music which results from the facile hand of a skilful performer."

This is, no doubt, absolutely true, and no instrument, however ingeniously contrived, can possibly act with as much precision as the skilful hand of a surgeon. There are, however, other instruments besides Civiale's which may be made to cut with almost equal precision; for instance, Mr. Berkeley Hill's urethrotome, in which there is a most ingenious combination of stretching and cutting. A wedge containing a small sheathed knife is made to pass down between two blades, separating them, as in Perrève's dilator. No force is used, but as soon as the wedge is felt to hitch against the stricture the concealed knife is protruded, and, cutting from before backwards, notches the stricture, which is kept steady and slightly stretched by the separated blades of the dilator. By this instrument the strictured portions only are cut, and, supposing it to be of moderate size, the cutting can be done with as much certainty and be as accurately limited to the stricture as if Civiale's instrument were used. Great stress is laid upon the complete division of

the stricture, and Sir Henry Thompson tells us that we need not fear freedom of incision. He has never seen any harm produced by it, and has "never had to regret a free and complete division."

These, then, are the great principles of treatment laid down in these lectures:—Always, whenever possible, use soft in preference to solid instruments. Treat every stricture by simple dilatation if you can. If that is not successful, or if there is great difficulty in the first introduction, employ continuous dilatation followed by simple dilatation; and if the stricture refuses to yield to these means, do internal urethrotomy, using some instrument by which the incision can be regulated accurately by the hand of the surgeon. Extraordinary means, such as external urethrotomy, puncture of the bladder, &c., can only be required in rare and exceptional cases. Auxiliary means, such as rest in bed, diet, and attention to the bowels, form valuable adjuncts to the treatment, but Sir Henry Thompson does not seem to expect so much from them as some surgeons. As to permanent cure, claimed by so many writers, past and present, for their own particular mode of treating stricture, Sir Henry Thompson does not believe in it. "The treatment has yet to be devised," he says, "which will remove absolutely and for ever the occurrence of recontraction in a patient once the victim of an organic stricture."—*British and Foreign Medico-Chirurgical Review, April* 1877, *p.* 368.

66.—A CASE OF STRICTURE ILLUSTRATING THE ADVANTAGE OF OCCUPYING A FALSE PASSAGE WITH A CATHETER TO ENABLE A SECOND INSTRUMENT TO BE PASSED INTO THE BLADDER.

(Under the care of Mr. TEEVAN, West London Hospital.)

John B., a police-constable aged 43, came under Mr. Teevan's care for stricture of the urethra in the out-patients' department on December 29th, 1875. The patient, when examined, was found to have a tight stricture situated five inches from the orifice, which would only admit a No. 2 catheter. As there was considerable enlargement of the prostate, Mr. Teevan passed a well-curved metal instrument. On January 3rd, 1876, the same catheter was introduced as on the former occasion. On January 5th, Nos. 3 and 5 were passed; and on January 8th, No. 6. On January 12th, Mr. Teevan, whilst trying to introduce a catheter, opened up some false passage or pouch, and could not pass any instrument, soft or metal. On January 15th, Mr. Teevan could not attend at the hospital; and Mr. Alderton, the house-surgeon, tried without success to introduce different instruments. On January 19th, as Mr. Teevan could not pass

any catheter, he put in practice the following manœuvre. He first of all took a medium-sized elastic cylindrical catheter and tried to pass it into the bladder. It went, however, into the false passage, where it was left. He then introduced a fine olivary elastic bougie, which entered the bladder. A small elastic catheter was then screwed into the bougie and made to follow it into the bladder, the first catheter being withdrawn as soon as the urine began to flow through the second one. No further obstacle occurred in the treatment of the case, and by February 20th the patient was able to pass No. 16 elastic olivary catheter with ease for himself.

Mr. Teevan observed that the presence of a false passage, or a sac left by an abscess, was always a troublesome complication of stricture, and in this country often rendered catheterism impracticable and an operation imperative, as English surgical works recorded no means by which the difficulty could be overcome, beyond the suggestion that, as false passages were usually situated in the floor of the urethra, they were best avoided by the employment of a metal catheter kept well pressed against the roof of the canal during introduction. But as false passages might be found in the roof of the urethra as well as in the floor, and an accurate diagnosis was not always possible, the suggestion was not of much value. Now, there were several resources open to the surgeon, the employment of any one of which would usually prove successful. In the present instance, he had practised the manœuvre originated by Mercier, which was founded on the simple fact that two bodies could not occupy the same space at the same time. If a full-sized cylindrical elastic catheter were introduced into the urethra, it would either slip into the false passage or pass on into the bladder. If it went into the bladder, all was well; but, if it passed into the false passage, it could be retained there whilst a small elastic catheter was introduced. If the first instrument completely filled the false passage, the second one would necesarily go into the bladder, as there was but one road open to it to follow.—*British Medical Journal, Nov.* 25, 1876, *p.* 681.

67—ON FILIFORM BOUGIES.

By Dr. J. C. Ogilvie Will, Surgeon to the Aberdeen Royal Infirmary.

So little notice is taken of filiform instruments by authors of standard works on Surgery, that—notwithstanding the powerful advocacy of leaders in surgery both in this country and abroad in works specially devoted to the consideration of surgical diseases of the genito-urinary organs, and in papers in the medical periodicals—it is not to be altogether wondered at

that they have not yet found favour with the great body of medical practitioners.

Four varieties of filiform bougies are met with, viz., the catgut, the English, the French, and the whalebone. Of the two first I have no personal experience, but those who may desire information concerning catgut bougies will find a valuable paper describing their mode of employment, by Prof. Rawdon Macnamara, in *Retrospect*, vol. lxxii., p. 234, and notices of the English instruments—bougies and catheters—may be found in Van Buren's work, and in Sir H. Thompson's Lectures, already alluded to.

The French filiform bougies are made of the same material as the well-known French catheters; some of them have a central wire of lead or copper to allow of their being bent at different angles, while others have a slender piece of whalebone in place of the wire to give them firmness. For my own part I prefer the simple French instruments without any addition, as, if it is desired to use a bougie bent to an angle, or if a stiffer instrument is required, whalebone will be found to fulfil both indications. Whalebone filiform bougies are thin, bristle-like strips of that material, and may be obtained with or without olive tips; they are extremely smooth and non-irritating. By dipping them in hot water they are rendered pliable, and their points may be twisted into any form desired, which they will retain on becoming cold.

French filiform instruments measure from one millimetre in circumference upwards, while whalebone bougies may be had of a still smaller size. Some are of the same thickness throughout, but most of them are made with the shafts thicker than the points, the tips measuring one millimetre, and gradually increasing to one, two, three or more milimetres as the proximal extremity of the instrument is reached. Flexible catheters which, from their minute size, may be regarded as filiform, are also to be had. I have lately received a supply of these instruments from Krohne and Sessemann, which they tell me are the smallest catheters yet made; they measure rather more than three millimetres at their points, and four at their shafts, and contain strands of very delicate wire in place of the ordinary stylets. They are thus considerably smaller than No. 1 English.

The mode of using filiform instruments hardly requires any notice, as it does not vary in any very important particulars from that followed in the case of other flexible instruments. A little difficulty is sometimes experienced in passing a filiform bougie to the strictured point, as its fine point is apt to become engaged in enlarged follicles so often present in urethræ which have been the seats of inflammations. This difficulty may be

easily overcome by first filling the canal with oil, as was done in Case 1, where the points of entanglement were numerous, and not easily avoided.

It may be observed that I used carbolic oil, a practice I now invariably follow when lubricating any urethral instrument, as my friend Mr. Chiene has proved to my complete satisfaction, that the danger of introducing germs into the bladder is not to be lightly regarded. When the bougie is fairly within the stricture its further progress may be suddenly arrested by muscular spasm, and it may be so tightly grasped that it may be momentarily impossible either to pass it into the bladder or to withdraw it. When working with so slender and flexible an instrument the use of force would be manifestly out of place, as the result would inevitably be fracture of the bougie or bending it in such a way as would utterly defeat the object in view; therefore the best rule to follow in such circumstances is to leave the bougie in position for a few minutes until the spasm ceases, when little difficulty will be found in guiding it into the viscus.

In some cases, however, after the stricture is traversed by the bougie, it may still be arrested before the bladder is reached, from its point becoming entangled in the meshes of the reticulated mucous membrane lying behind the contraction; if this happens, the bougie should be slightly withdrawn, and a fresh attempt to lodge it should then be made, the instrument being advanced in a rotatory manner, the twisting movement rendering the probability of entanglement much less likely than if it be simply pushed straight onwards. Filiform bougies with angular points are chiefly useful in assisting one in finding the orifice of a stricture when it is placed to one side instead of being in the mesial line; when the strictured point is reached the bougie should be gently twisted from side to side until the point engages itself in the opening, when it may be pushed through the strictured portion of the canal into the bladder. Whalebone bougies with twisted or corkscrew points are useful in traversing long and tortuous strictures. The slender catheters just mentioned are warmly recommended by Sir H. Thompson, on account of their ability to transmit urine, and thus to assure the operator that the instrument has really reached the bladder.

When penetration of a tight stricture has been effected, and the filiform instrument lodged in the bladder, it may be utilized in various ways, on which I shall not enter at any length, but content myself with enumerating them.

1. Filiform instruments may be obtained with little female screw-caps fitted on to their proximal extremities, which can be attached to flexible catheters fitted with male screws at their points. Mr. Teevan's instrument, so constructed, is a valuable

one, and may be used with good effect when stricture is complicated with urgent retention of urine; the catheter, after attachment, being guided into the bladder by the bougie, which curls up in the viscus. After the bladder is completely emptied, the catheter portion of the apparatus should be withdrawn and detached from the bougie, the latter being left in the urethra for the purposes of continuous dilatation, or to allow of the reattachment of the catheter should it be again required, American surgeons sometimes use fine silver instruments screwed on to filiform bougies in the same manner. Of an instrument, of which he gives a woodcut, and which is very similar to that sold in this country as Teevan's, Phillips says, "Cet appareil, d'une grande simplicité m'a été très utile."

2. Whalebone filiform instruments may be used as guides over which a catheter, either silver or flexible, open at its point, may be passed through the stricture; or a tunnelled catheter, *i.e.*, a catheter with a little bridge of metal on its convexity and close to its point may be threaded over a filiform instrument, and thus be conducted to the bladder.

These latter methods, though occasionally useful, will not probably receive general adoption, as they necessitate the use of much longer filiform instruments than are generally employed. Van Buren and others hold that the guide should be two feet long so as to allow of gradual retraction while the catheter is being passed onwards towards the bladder, lest the whalebone should get doubled up and broken in the canal. These long and somewhat unwieldy strips of whalebone are much more difficult of management than the ordinary-sized filiform instruments.

3. Filiform instruments may be attached, as already described, to instruments used for divulsion, or internal urethrotomy, or to the staff used in Syme's operation.

4. They may be used as guides in external perineal urethrotomy, the mode of operation originally suggested by Van Buren, and perfected by Gouley, being followed, or the modification described by Teevan being adopted (*Retrospect*, vol. lxxiv, p. 214.)

5. If the crisis of retention of urine is not very urgent, a filiform bougie, *per se*, may be left in, as it will, in a very short space of time, act as a guide to the urine, which will pass by its sides, and the patient will soon be enabled to empty his bladder in this way without the aid of a catheter. Phillips employs a somewhat similar mode of treatment in cases of retention; he introduces a whalebone filiform instrument, leaves it in for a few minutes, then withdraws it partially, the with-

drawal being followed by a squirt of urine, and he goes on repeating this manœuvre until the contents of the bladder are sufficiently evacuated to relieve the agonies of the patient, or until the urine begins to flow by the side of the bougie; and of this he says, "Cette manière d'attaquer la rétention d'urine est longue, peu brillante et peu chirurgicale, selon le sens attribué â ce mot; mais elle est sûre, exempte de danger, et elle donne un résultat assez complet, puisque, en peu de temps, elle fait cesser les douleurs, quoiqu'on n'ait pas évacué la totalité de l'urine." That micturition is readily accomplished during the residence of a filiform instrument within a stricture may be seen from the following cases, and observations to the same effect are set forth in a paper by Mr. Furneaux Jordan (*Retrospect*, vol. lxxiii, p. 201). In Case 2, mentioned below, the patient had a better stream soon after the tying-in of the bougie than he had had for many years. Another advantageous mode of employing filiform instruments does not come under any of the foregoing heads, viz., their usefulness in cases of stricture complicated by false passages. In cases of this kind filiform bougies may be used to fill the false passages, first one instrument being passed in and left in position, then another, and so on. After all the openings are filled with bougies, each one should be tried in turn until that one lying in the mouth of the stricture is found, which will be at once known by its being grasped, then the other instruments are withdrawn, and the remaining bougie pushed into the bladder.

Such, then, are the main uses of filiform bougies, and from what has been pointed out, I think that it must be ceded that they are at least worthy of more notice than they have yet received at the hands of British surgical authors; and it does not seem to me to demand a great amount of comprehension to understand that a minute flexible instrument will worm its way through a stricture with greater ease to the surgeon, *and far less risk to the patient*, than an inflexible metallic probe, be its point never so fine. In the case of a master such as Liston was, surgical miracles may perhaps be accomplished; but this cannot be expected of those to whom the treatment of stricture is somewhat a rarity, and on them I would strongly urge a trial of filiform instruments, feeling assured that they will speedily become converts to the doctrine that flexible instruments are to be infinitely preferred to metallic, and that impassable stricture is more a thing of the past than of the present. Thus they will be enabled to endorse Mr. Syme's statement "that if the urine passes out, instruments may always, through care and perseverance, be got beyond the contraction."—*Edinburgh Medical Journal, April* 1877, *p.* 882.

68.—SIR H. THOMPSON ON STONE IN THE BLADDER.

(From a Review by the EDITOR of the BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.)

Sir Henry Thompson treats of this subject from its very beginning, one lecture being devoted to the early history of calculous disease and the treatment best adapted for its prevention. Excluding phosphatic calculi, which are, of course, always of bladder origin, and can only be prevented by local means calculated to prevent decomposition of the urine and consequent cystitis, nineteen out of twenty calculi are composed of uric acid, and the preventive treatment recommended is consequently that usually adopted in the gouty diathesis; but Sir Henry Thompson differs from many in his absolute condemnation of alkalies, including Vichy water, in the treatment of this condition. On this subject he says—

"Now, it is quite true that with alkalies, provided enough be taken, these deposits (urates and uric acid) will disappear; the uric acid will no longer be deposited; the urine will become less irritating; the annoying symptoms will be diminished or got rid of; and, of course, the patient is very much pleased with this new condition of clear urine and disappearance of all deposit. And you will say, 'What more can be desired?' This: you have merely made his enemy disappear, but he is by no means rid of its presence.

"You have not checked the acid formation. The uric acid is there as much as ever; but the uric acid and the urates are soluble in alkali, and you have only made them invisible. You really have the same condition as that of the fabled ostrich, which is said to put its head in the bush when pursued by hunters, and, no longer seeing them, to believe itself secure."

Not only this, but Sir Henry Thompson believes that large quantities of alkali habitually taken act injuriously, and tend possibly to increase the tendency they are given to check. It is upon attention to diet and to the employment of "such agents as will stimulate the excretory action by the primæ viæ without depressing vital power," that, Sir Henry Thompson relies in the treatment of the uric-acid diathesis. The best agents of this kind he believes to be the various purgative or aperient mineral waters, such as Friedrickshalle, Püllna, Carlsbad, and Hunyadi János, and, instead of sending a patient suffering from gravel to Vals or Vichy, he would recommend a course at Carlsbad. But, doubtless, Sir Henry Thompson is conferring a great benefit upon patients suffering from these affections when he points out that a course of these waters, if taken at home, combined with judicious diet and abundant fresh air and exercise, will do as much good as if taken on the

spot, and probably more than if drunk in the enormous quantities usually recommended at the watering places. It is an interesting fact which Sir Henry Thompson states, that he has verified by experiment that these waters possess more active powers than artificial solutions of similar chemical composition. He says—

"If you will reduce by careful evaporation, as I have done, such mineral waters to their pharmaceutical condition of crystallised salts, you will find them possessing little if any more power than similar salts as obtained by the ordinary processes, and met with in every chemist's shop. They no longer do their work on the same terms as when administered in the original water before they were separated by evaporation."

Supposing the preventive treatment to fail, the next point is early diagnosis of the stone. In the present day it seems almost a disgrace to our profession that stones should ever reach a large size, and yet how frequently it happens still that patients are allowed to go on for many months, or even for years, without being sounded. In every case of frequency of micturition, especially in patients past middle life, and perhaps of a gouty tendency, stone should be the first thought of the surgeon, not, as is too frequently, the last, and only arising when months of medical treatment have proved abortive and the symptoms have gradually but steadily increased. As Sir Henry Thompson points out, the pain after passing water is in the early stages "a mere passing sting." If even this amount of pain occur, with frequency of micturition greater by day than by night, and possibly a minute drop of blood after riding on horseback or in a carriage, the patient should be sounded without delay. Sir Henry Thompson, moreover, calls attention to the fact that stones sometimes get overlooked for a time, the patients getting into the hands of some one whom the ordinary etiquette of the profession forbids to use a sound or catheter, where they remain under treatment for gouty cystitis, catarrh of the bladder, and such like, till, their symptoms becoming more severe, a surgeon is called in.

The author recommends that the physician should use a sound; we think, on the other hand, it would be much better if all cases in which the bladder is evidently the seat of the disease he should at once hand the patient over to the surgeon, for, taking bladder diseases as a whole, internal remedies play but a small part in their cure.

A very interesting lecture in Sir Henry Thompson's work is devoted to the history and prospects of the solvent treatment of calculus. On this subject the only experiments of any value are those of Dr. Roberts, of Manchester, and it may be said that he has conclusively shown that there is no hope of any

success except in dealing with very small uric acid calculi, not coated with phosphates, and even with these the treatment is slow and doubtful, no recorded case ever having proved successful. Such stones can be certainly removed by one or two applications of the lithotrite, and the patient rid of his trouble in a few days; and the solvent treatment should, therefore, we think, be finally abandoned as not only useless, but dangerous, as every day a stone lies in a patient's bladder the difficulties and dangers of the cure are increased.

The operation of lithotrity has now been before the profession in a practical form for over fifty years, and we believe it may be said without fear of contradiction that it has reached its limit of perfection. The instruments employed have been reduced to the minimum size consistent with safety; they are so designed that it is almost impossible for the coats of the bladder to be seized between the blades, and the mechanism by which the crushing force is applied to the stone is so simple that any improvement is scarcely conceivable. The only question before the profession at the present time is the limits of its applicability, and the vast experience of Sir Henry Thompson, which has been at various times laid before the profession, goes far towards settling this point also. His views on the subject he summarises by saying "in general terms, that all the cases above puberty are to be crushed, with certain exceptions." These exceptions are, an oxalate of lime calculus over one inch in diameter, and a uric acid or phosphatic calculus two inches or more in its long diameter. Stricture of the urethra, enlarged prostate, atony of the bladder, or irritability and extreme sensibility of the bladder, are not in Sir Henry Thompson's opinion sufficient reasons for abandoning the operation and falling back on lithotomy. Stricture of the urethra he dilates till the lithotrite can be introduced; in enlarged prostate the stone can be reached to the last fragment by reversing the blades of the lithotrite; in atony and in enlarged prostate the fragments may be removed by Clover's aspirator, and in extreme irritability of the bladder the condition will be relieved by crushing the stone. Thus it will be seen that increasing experience has led the author rather to an extension of the operation to cases formerly considered unfit than to a limitation of it within narrower bounds. The size of the stone seems to be almost the only counter indication recognised by Sir Henry Thompson, and, as medical science and education advance, stones of the sizes above mentioned should be almost unknown, and accordingly he says, "I hope you will live to see the day when lithotomy for adults will be one of the rarest operations in surgery. I do not suppose I shall; but I do expect to live to see one thing, and that is, lithotomy becoming less common

year by year." These, then, may be taken as the views of the greatest living advocate of lithotomy; on the other hand, many surgeons of eminence still believe that it is better practice to cut for every stone, however small. They urge against lithotrity, that although the patient can be easily rid of his stone, he is usually left with an almost incurable condition of chronic cystitis, with a tendency to decomposition of the urine and consequent formation of secondary phosphatic calculi. There is no doubt this accusation is in part true, but in reply it is urged that when this unfortunate result occurs it is not in cases in which before the operation the bladder was tolerably healthy and the stone small, but in old subjects with much previous bladder mischief, and in whom perhaps the stone was originally phosphatic, and merely secondary to the bladder condition. Now, these cases are markedly unfavourable for the cutting operation, and the advocates of lithotrity say it is better for a man to live with cystitis than to die from lithotomy.—*British and Foreign Medico-Chirurgical Review, April* 1877, *p.* 371.

69.—WHEN IS THE CATHETER TO BE USED FOR HABITUAL RETENTION FROM HYPERTROPHIED PROSTATE?

By Sir Henry Thompson, Surgeon Extraordinary to H.M. the King of the Belgians; Emeritus Professor of Clinical Surgery in University College Hospital.

There is a question which arises in the treatment of these cases which sometimes demands careful consideration. It is: "What are the circumstances which should determine us to commence habitual catheterism for a patient whose hypertrophied prostate prevents him from emptying the bladder by his own efforts?" I reply that there are at least two principal facts relating to the local symptoms which must be noted in arriving at a judgment for any particular case. Firstly, we must know the amount of "residual urine" habitually present—that is, the quantity left behind in the bladder after the patient has passed all he can by his own efforts; and, secondly, we must observe the degree of frequency, by day and by night, with which he passes water, but especially during the latter period.

In reference to the first particular, the amount of "residual urine," you will of course not always judge from one trial of the catheter. Where there is nothing to disturb the function of micturition—and it is very easily disturbed, as by the presence of a stranger, or by the requirement to perform the act for purpose of experiment when a natural want is not present—the amount of urine left in the bladder is pretty uniform with most patients; the conditions just referred to often temporarily

impairing the expulsive power, and rendering therefore the residual portion on the occasion of examination rather larger than usual. This understood, let us suppose a case in which eight ounces always remains behind. That quantity suffices, in my opinion, to make it desirable that the patient should at once commence the daily use of the catheter. But you may find a much smaller quantity; and you are entitled to ask me—indeed, I expect you to do so,—"What is the point in regard to quantity at which the line is to be drawn? When can I say, With this quantity a catheter is quite unnecessary; or, With such a quantity there is no doubt it must be used?" No answer can be given to such a question. The data from which to form a correct judgment are not contained within the terms of the proposition. Other facts are to be ascertained. I have heard it laid down, indeed, as an axiom, that so long as the urine is clear, no matter what the quantity retained, no instrument ought to be employed. A certain amount of *à priori* reasoning may be urged in behalf of such a rule, but it will not bear the test of large experience. The problem presented for solution in this, as in most other cases where surgical interference is imminent, is far too complex to be solved by one unvarying rule. Like the statement respecting quantity referred to above, the single fact that the urine is clear does not suffice to govern your decision. A large quantity of residual urine, much more than a pint, may exist, clear and acid in certain cases, but which, as we shall hereafter learn, ought certainly to be drawn off by catheter.

But let us see what this rule of never withdrawing the urine while it is clear means or involves. It means neither more nor less than waiting for the occurrence of chronic cystitis before we use an instrument! No other inference is possible. And why should we wait for chronic cystitis?—a condition which of all others it is highly desirable to avoid in an old and already incompetent bladder, leading as such an affection naturally does to thickening of tissues and loss of extensibility on the part of the organ. Is it not, on the contrary, the very condition we desire to avoid, and do mostly avoid, by commencing the use of the catheter at a sufficiently early period? Of course I know full well that in past days, when catheterism necessarily meant the use of the large metallic instrument, often painfully passed, to say the least, chronic cystitis was an ordinary and frequent result of catheterism. But it rarely is so now, with the soft and flexible instrument of moderate size, if only it is used at an early period in the case, and before considerable accumulation has taken place; the removal of a large quantity being mostly, I do not say invariably, followed by local and general disturbance. For it is not very common to find a patient whose

residual urine has, from neglect of catheterism at an early period, reached the quantity of twenty ounces or more, who does not suffer somewhat severely from both chronic cystitis with purulent urine, and febrile attacks with resulting debility, whenever the daily use of the catheter has to be commenced. Furthermore, at this advanced stage of chronic retention, a slight accident of some kind readily occasions complete retention, or nearly so; and then it is no longer a question of using or not using the instrument, since the condition is now one which imperatively demands a catheter. In these last-named circumstances, chronic cystitis is almost sure to follow—an occurrence which most probably would not have happened had there been an earlier resort to the instrument. And so it happens, in the management of these cases of continued and chronic retention due to slowly advancing hypertrophy of the prostate, that the longer the use of the catheter is postponed after the early stage of the malady is passed, the worse will be the symptoms. And it happens also, unhappily, not seldom, that these serious symptoms following the use of the catheter bring undeserved discredit on the surgeon who first employs it—a discredit really attaching solely, and very gravely too, to the adviser who unwisely prevented an early resort to its aid. So that I beg you to understand that with a quantity of habitually retained urine amounting to eight or ten ounces, whether it be thick or clear, there is no question in a vast majority of cases that the time for the catheter, say, at least once, probably twice a day, has arrived.

But what other circumstance has also to be taken into account? A very important one—viz., the frequency with which the patient passes urine, and which differs greatly in different cases. It is much more to the purpose for your decision to note whether the patient is disturbed six times in the night or only twice, than whether his urine is clear or cloudy, or even whether the residual urine amounts to four ounces or to twelve. If you find him affected by loss of rest—one of those things which sap the foundations of life in elderly men,—pass the catheter the last thing at night for him, and mark the result. If he obtains four or five hours of continuous sleep after the bladder has been emptied—a common occurrence,—you have reason enough for persevering, and he will learn to use the instrument himself every night, and will be exceedingly grateful to you for the relief he has obtained. So also the avoidance of pain and spasm by this treatment—a result which is often to be noted—makes it highly desirable, whether the quantity drawn off be small or large, transparent or clouded by mucus. Taking into consideration these different phenomena, which vary so largely in different constitutions, you will have no difficulty in arriving at a

correct judgment for each individual case, provided you give to each sign or symptom its due importance, and do not rest your decision on any one, unless that one—in regard of quantity, for example—is sufficiently marked to leave no ground for doubt as to your course.—*Lancet, Jan.* 6, 1877, *p.* 1.

70.—ORCHITIS TREATED BY PUNCTURING THE TESTICLE.

(Under the care of Mr. MACNAMARA, at the Westminster Hospital.)

The treatment of acute orchitis by means of puncturing the testicle having within the past twelve months attracted a considerable amount of attention, the following notes, for which we are indebted to Mr. George Shaw, will doubtless prove of interest. The subjoined cases, as far as they go, certainly seem to present very satisfactory evidence of the value of puncture, while, according to Mr. Macnamara's wide experience, such instances are by no means rare.

Case. 1.—H. C., aged forty-one, a gold refiner, was admitted on October 17th last with acute inflammation of the left testicle. He was a temperate man and a hard worker, but out of health in consequence of being consequently exposed to nitro-hydrochloric acid fumes. On October 11th he strained himself while at work, and shortly afterwards his left testicle became swollen and very painful, so that he was quite unable to continue his work, and, as the treatment he received at his house did not relieve him, he was taken into the hospital. Ice was kept constantly applied to the inflamed gland, and the ordinary saline purgatives were administered. Under this treatment the symptoms subsided, but on the 24th, without any known cause, the orchitis returned, and, on the following day, during his visit to the hospital, Mr. Macnamara ran a grooved needle into the testicle, and allowed a few drops of serous fluid to escape externally along the groove, after which the instrument was withdrawn. The relief was both immediate and permanent; the inflammatory symptoms all passed away, and the patient left the hospital on November 3rd perfectly cured.

Case 2.—Thomas W., aged thirty-five, was admitted on November 4th, suffering from long-neglected gonorrhœa and acute inflammation of the right testicle, the latter having come on suddenly on October 29th, from which time he had been in very great pain. Immediately after admission the house-surgeon, Mr. Poynder, passed a grooved needle into the testicle, and, after a small quantity of fluid had escaped externally, withdrew the needle. The patient alleged that within five minutes the pain had entirely gone, and did not return again from that time. He left his bed on November 12th, and left the hospital cured on November 20th.

In reference to these cases Mr. Macnamara remarked that they were fair examples of the effect produced by puncturing the testicle in acute orchitis. So far as he was concerned he was unable to determine in any given case if the inflammation was confined to the epididymis, or affected only the proper structure of the testicle; but it seemed to him scarcely probable that inflammation, if attacking one of these organs, would not extend to the other, and under any circumstances it followed, almost of necessity, that an effusion of fluid from the distended bloodvessels would escape into the tunica vaginalis, and perhaps, also, into the tunica albuginea. Every surgeon who had punctured the testicle in acute orchitis must have observed that the escape of a small quantity of fluid along the groove of the needle was not unfrequently followed by instant relief of the pain and a diminution in the hardness of the testicle, and it had always appeared to him that the relief was analogous to that afforded by diminishing the tension of the eyeball in acute glaucoma. Mr. Macnamara further remarked that he could claim to speak with some degree of confidence on this subject, for, some years ago, while riding, he was thrown forward on the pommel of his saddle, and injured his left testicle. Symptoms of orchitis soon set in. Happily having been informed by his friend, Dr. Herbert Baillie, only a short time previously of the case of an artillery officer whose testicle had been punctured for orchitis after the plan recommended by Mr. Henry Smith of King's College, Mr. Macnamara got Mr. Culcliffe to run a grooved needle into the inflamed and injured testicle. The relief in his own case not only instantaneous, but was permanent, and for these and other reasons he said he had never hesitated to employ the same treatment on his patients. He himself had never seen any but favourable results follow this mode of treatment, though, of course, he was not prepared to say it was always curative. He added that he felt himself under a personal obligation to Mr. Henry Smith for having introduced into modern practice the plan of puncturing the testicle in cases of acute orchitis, and he could with confidence recommend his pupils to follow this treatment in similar cases, because there are few diseases in which pain can be more effectually and speedily removed.—*Lancet*, *Jan.* 13, 1877, *p.* 50.

71.—URINARY CALCULI.

(Notes from Mr. CHRISTOPHER HEATH'S Clinique, at University College Hospital.)

[It is a curious fact that the risk in lithotomy is almost in proportion to the weight of the patient. Very fat men have a poor chance.]

It has been a question whether uric acid is secreted by the kidney or merely excreted, but the great preponderance of opinion now is towards the latter opinion. When the stone is very small, just forming in the kidney, there may be no symptoms other than some aching of the loins; later, pyelitis may come on, and blood and pus may be passed. Then the stone may descend and block up the ureter; and if one kidney only be affected, the patient's urine will for a time be clear: the pus will be retained with serious consequences to the kidney structures, or the obstruction pass on, with an attack of "renal colic", and reach the bladder. Now, if the stone be small, it may be at once passed out, and the best plan to help this is that described by Sir T. Watson. Have the patient's bladder full, place him on hands and knees so as to keep the neck of the bladder low, let him for a time prevent the passage of urine by grasping the penis, and then suddenly take off the pressure, and strain forcibly to empty the bladder. If, on the other hand, the stone do not leave the kidney, that organ may either become hollowed into a great cyst, or atrophied round the obstruction, and the patient will live with one kidney. Again, the stone may stop in the urethra; the symptoms of pain and obstruction may be very severe, but easily explained by the passage of a probe. This urethral obstruction is, perhaps, most common in children; and, if overlooked, as it not very infrequently is for a time, may lead to extravasation of urine; in fact, Mr. Cooper Forster says he has never seen extravasation in children independently of calculus—not that it need wholly obstruct, but gradually ulcerate through.

Now a nucleus, being in the bladder, always *grows*. In the case before us, symptoms had existed for three years, but the stone had only been detected one week. It ought to have been (and now-a-days generally is) found earlier, and the issue would very likely have been different. You may have a stone with an uric acid nucleus, and a ring of phosphate succeeded by another ring of acid, and so on; the explanation being that the original irritation causes alkaline urine and phosphatic deposit; then a period of rest and treatment restores the normal reaction for a time. The prominent symptoms of vesical stone will be (1) *frequent micturition*. Very few people notice how often they pass urine; it is usually about six times in twenty-four hours; but, if they have to rise in the night, they notice it, for it breaks their rest. (2) *Pain*, often referred to the penis, and mostly after the bladder is emptied; (3) but the only real proof is to *feel the stone*. The sound used may be solid, or hollow, but now we always use one with this small curve: be quite sure you get into the bladder; it is possible to stick in the prostate. You will make sure by injecting water, and never forget to feel

behind the prostate. It was a saying of Coulson's, "In every case of enlarged prostate, suspect a stone." Several stones may be pouched. I remember being disappointed after crushing a stone, to learn that my patient had been cut by another surgeon a few months afterwards and had five stones removed; but three months afterwards he was cut again, and two more removed, and the same thing happened even a third time. He is now well, and I conclude these stones were at the time retained in a vesical pouch." Mr. Heath then described lithotrity with Sir H. Thompson's lithotrite (and the use of Clover's bottle for washing out the bladder), and afterwards lithotomy, with a grooved curved staff. He sometimes performed the median operation, but generally the lateral; it gave more room, though it more injured the soft parts, and had more risk of bleeding. The rectum should be avoided, though it was sometimes injured without any bad results, and Mr. Lloyd of St. Bartholomew's Hospital formerly practised a recto-vesical operation; its results were not very good. "It is well to cut low to avoid the bulbar artery, though some able surgeons cut rather high. It is all but impossible to cut the pudic artery, and the risk of hemorrhage is not great. If there be fear of it, you can put in a catheter surrounded by a linen plug—a 'tube en chemise.' Having introduced the staff, the assistant holds it with his right hand, and draws up the penis with his left—a most important matter—aud then what you have to do is to reach the groove; having done so, you might still miss the bladder, but follow this rule of Fergusson's, *raise your hand and the scalpel-handle*, and you will get all the advantages of a straight staff, and will readily pass along the urethral canal. How Liston held his knife in the deep cut has been debated. I think he had two methods; in the later part of his career he certainly held it in the ordinary way, with the finger on the back, but earlier, I believe, he placed the finger below the edge of the knife; and this is really rather a good plan for a beginner to follow. Sometimes, as in the case before us, you cannot really reach the bladder with your finger; then put in a director, withdraw the staff, and dilate the parts. Having touched the stone, and taken up your forceps, be careful not to take the hooked end in your thumb, for it will probably slip, and you will be tempted to begin to work the forceps with two hands—a plan very much to be avoided—but place your thumb in the ring, the fingers in the hook. Of other plans of performing the operation, we may mention Dolbeau's, of crushing the stone from the perineal opening—but it is not satisfactory, and has not made way in this country.—*British Medical Journal, Nov.* 4, 1876, *p.* 588.

72.—ON THE CURATIVE INFLUENCE OF AN EXCLUSIVE MILK DIET IN SOME CASES OF INFLAMMATION OF THE BLADDER.

By Dr. GEO. JOHNSON, F.R.S., Professor of Clinical Medicine, Senior Physician to King's College Hospital.

[An exclusive milk diet is of use in various forms of disease, thus some cases of diarrhœa and dysentery may be completely cured by it without the aid of medicine. The diarrhœa of typhoid is again generally benefited by it, and in acute Bright's disease we often see a speedy disappearance of the albuminuria under the influence of rest in bed, a few warm baths, and copious libations of milk.]

I desire to direct attention especially to the beneficial influence of an exclusive milk diet in the treatment of some cases of cystitis. No case of this kind has occurred recently in my hospital practice, but within the last two years I have seen elsewhere several cases in which the curative influence of a milk diet had been very remarkable, and I propose now to give a few particulars of three illustrative cases, taking them in the order in which they occurred.

Miss D., aged seventeen, consulted me first on January 22nd, 1875. The following history of the case was given in a letter from the patient's mother. "It was in March, 1874, that my daughter first noticed an irritability of the bladder, disturbing her many times in the day, and three or four times every night. In June she began to suffer pain, especially at night. She was treated by various medical men, but became daily worse, the water containing blood, pus, and albumen. On the 14th of October she was sounded for stone, and its absence proved, but all the symptoms became much aggravated. During the succeeding three months she suffered, at intervals varying from two to three days, severe attacks of pain over the bladder, lasting from two to five hours, during which she passed water every two, three, or five minutes. Very hot hip-baths and opium pills afforded temporary relief. I remarked that fish, and more especially oysters, as food, always brought on a severe attack of pain and irritation. On Jan. 22nd, 1875, being then seventeen, she came under your care." She was then suffering from severe pain over the bladder, and from very frequent and painful micturition, so that her life was rendered miserable. The urine was acid, and contained a considerable amount of pus. All the symptoms appeared to be the result of inflammation of the bladder, but her mother had been positively assured that there was disease of the kidneys. She had taken a great number and variety of drugs, but opiates alone had afforded any temporary relief. I advised a trial of an exclusively

milk diet, warm hip-baths at night, and an occasional opiate draught, which she had been in the habit of taking for the relief of pain. She also took occasionally some pills containing camphor and extract of henbane. I saw the patient five times during the months of January, February, and March. During that time the milk diet was continued, and there was a gradual improvement. After March 22nd I did not see her or hear of her again until Sep. 12th, when I received a letter from her mother, in which she says: "I am sure you will be glad to hear that your young patient is now quite restored to health, and if on any day she does not feel quite so well, a return to *exclusive* milk diet for twenty-four hours makes all right again. She began your treatment in January, and although the improvement was very slow it was continuous. She spent the month of August at the seaside, bathed and swam without any ill-effects, and she is able now to resume all her old occupations. Her food is still principally milk and biscuit, plain meat once a day, very seldom some green vegetable, never tea, coffee, beer, or wine. I give you these details believing they will interest you; and because we have had to feel our way so cautiously in the matter of food." After receiving this letter I requested the writer to call on me with her daughter, and to bring me a specimen of my quondam patient's urine, which I found to be in every respect quite normal, while the young lady reported herself, and looked, in perfect health, and free from all local irritation.

Mr. C. B., a country gentleman, aged forty-eight, consulted me on Oct. 13th, 1876, on account of frequent and painful micturition, which had troubled him for two years. He stated that two years ago he contracted a gonorrhœa, for which, at the commencement of the malady, he used an injection of zinc and morphia. This quickly stopped the discharge, but, he then began to suffer from pain in the bladder and frequent calls to pass water, the urine being turbid with mucus, and for several weeks tinged with blood. The frequent painful micturition and turbid urine had continued to the present time. He generally has to pass water every hour both day and night. The pain and irritation are much increased by walking and driving, so that in going about he has to wear an india-rubber urinal. The urine brought to me was turbid with puriform mucus, of normal density, of acid reaction, and deposited some red sand. It was slightly coagulable by heat and acid, the albumen being probably derived from the pus; no tube-casts were present. He had consulted two eminent London surgeons, one of whom had sounded him for stone with a negative result; but the prostate was said to be enlarged. The symptoms had continued unabated, notwithstanding the various remedies

which had been employed, temporary relief only having been obtained by the use of opium suppositories. Up to the time of his first visit to me he had continued to take wine at his meals rather liberally.

I advised him to take cold or tepid raw milk exclusively—no other food or drink,—and I prescribed no medicine. He returned to his home in Yorkshire on the day after his visit to me, and a few days afterwards I received from him the following report, dated Oct. 20th :—

"I consulted you on Friday, the 13th October. I returned home on Saturday, the 14th, and since the middle of that day I have taken nothing but milk, neither solid nor liquid, as you prescribed. From the very beginning of this diet—that is, from the morning of Sunday, the 15th—I have felt no traces of the inflammation. It has been difficult for me to realise its sudden and (for the time) total disappearance. I twice yesterday drove three miles at a time in a common cab without pain or disturbance. Up to the 14th the irritability had been as bad as ever, and on the afternoon of the 13th I had been in very severe pain while driving a short distance in London. I write to ask how long I ought to continue the milk diet if the irritability does not return. The water is clear, but still leaves a slight cloud after standing in a glass bottle. I am in no hurry, and will follow your instructions strictly. I shall be glad to be liberated from the milk diet, but I prefer it to the inflammation." In a letter dated Oct. 22nd he says: "The necessity for making water very frequently (about once an hour) ceased along with the inflammation or irritability. I have since made water in the daytime at irregular intervals, but scarcely, if at all, more frequently than when in good health. Yesterday I walked three miles, and sat down for an hour at the end of the walk, without having to make water, and to-day I have also taken a fair amount of walking exercise without any uneasiness. After the length of time that I have been practically a cripple, this seems only too good to last." On the 23rd I received by rail a sample of his urine, which deposited only a scanty sediment of mucus, and contained no albumen. I then advised him by letter, while still abstaining from alcoholic liquors, to take in addition to his milk some plain solid food—fish, game, chicken, or mutton,—with some cooked vegetables, and for breakfast an egg with bread-and-butter. On Oct. 27th, exactly a fortnight after his first visit, he called and reported himself quite well. He had been four hours and a half in the train the day before without having once to pass water; he can hardly realise the fact that he is free from all discomfort. The urine is as clear as sherry, and entirely free from mucus. He returned home, and I received from him a note, dated Nov. 6th, in which

he says: "Until last Friday, the 4th, I continued to abstain from all alcoholic liquors, as you advised. On and since that day I have drunk wine at dinner only, as usual. This has had no bad effect to my knowledge, and I find myself at present perfectly well." He ends with an expression of thankfulness for the benefit derived from the treatment.

I have seen a considerable number of cases of rapid recovery from *recent acute cystitis*, where the disease has been promptly and judiciously treated, but I never before saw so rapid and complete a cure of severe cystitis of *two years' duration* as occurred in this case. The result surprised and gratified me almost as much as it did the patient. The *modus operandi* of the milk is sufficiently obvious. The urine is largely diluted with water, and rendered mild and unirritating by the digestible nature of the food; the bladder, therefore, being comparatively undisturbed by its contents, reverts to its normal condition, the inflammation of the mucous membrane subsides, and the morbid secretion of puriform mucus ceases.

The Rev. J. W., aged twenty-nine, a curate in a manufacturing town, consulted me on Nov. 2nd on account of frequent micturition and a painful feeling of irritation extending from the bladder to the end of the penis. He passes water almost every hour. These symptoms have troubled him for the last three months. He has a weak stomach, and he believes that his bladder trouble was excited by his having drunk one night two glasses of beer at supper. The urine was acid, turbid with pus, and slightly coagulable by heat and acid. I advised him to try a diet of milk exclusively, and to take no medicine.

Four days afterwards, on Nov. 6th, he called and said that he had been taking a gallon of milk daily, and that since the second day the feeling of irritation and the frequent calls to pass urine had ceased.

Nov. 10th. He has taken, in addition to milk, mutton, chicken, and rice. There has been no return of irritation, and he feels quite well. The urine is much less turbid, but it still deposits a slight cloud of mucus. He was to return to his work on the following day.

On Nov. 23rd he sent me a specimen of urine, which I found entirely free from mucus and in every respect quite normal; and in a letter dated Nov. 24th he says, "My health is, I think, better now than it has been for many months past, and my bladder does not seem to be affected in the slightest degree."

Perhaps a few hints as to the mode of giving milk in these cases may be useful. The milk may be taken cold or tepid, and not more than a pint at a time, lest a large mass of curd, difficult of digestion, form and collect in the stomach. Some adults will take as much as a gallon in the twenty-four hours.

With some persons the milk is found to agree better after it has been boiled, and then taken either cold or tepid. If the milk be rich in cream, and if the cream disagree, causing heartburn, headache, diarrhœa, or other symptoms of dyspepsia, the cream may be partially removed by skimming. One reason amongst others for giving the milk, as a rule, unskimmed—that is, with the cream,—is that constipation, which is one of the most frequent and troublesome results of an exclusively milk diet, is to some extent obviated by the cream in the unskimmed milk. As a rule, it is unnecessary, and, therefore, undesirable, to add bread or any other form of farinaceous food to the milk, which in itself contains all the elements required for the nutrition of the body. When the vesical irritation and catarrh have passed away and the urine has regained its natural character, solid food may be combined with the milk, and thus a gradual return may be made to the ordinary diet, while the effect upon the urine and the bladder is carefully watched.

There are some patients with whom, unfortunately, milk in any form, and even in small quantities, so decidedly disagrees that it is for them as unsuitable a diet as any other form of indigestible food would be.

If I might venture to give a hint to my surgical colleagues and friends, I should say that an exclusively milk diet would probably be found very suitable, for most patients, during the first few days after the operation of lithotrity, the object being of course to lessen, as much as possible, the inflammation and catarrh resulting from the mechanical irritation of the mucous membrane of the bladder.

I have recently seen two cases in which the vesical irritation and catarrh resulting from a stone in the bladder were much mitigated by the milk diet, the patients being thereby brought into a more favourable condition to undergo successfully, the one the operation of lithotomy, the other that of lithotrity.—*Lancet, Dec.* 16, 1876, *p.* 847.

73.—ON A CASE OF VARICOCELE TREATED BY RICORD'S OPERATION.

By Dr. J. C. Ogilvie Will, Surgeon to the Aberdeen Royal Infirmary.

Although the following case of varicocele presents no features of interest as regards its causation and symptoms, yet on account of the occurrence of an accident in the course of its treatment it seems to me to be an instructive one and not unworthy of notice, and it affords me an opportunity of directing attention to a slight modification of the method originally suggested by Ricord for the cure of varix of the spermatic cord. For the notes of the case I am indebted to Mr. W. R. Tough.

W. M., aged twenty-six, baker, suffering from varicocele, was admitted into Isaac ward on Jan. 31st, 1876. He stated that about three years ago he received a blow on the left testicle, which was followed by considerable swelling and pain, but which disappeared under treatment in about a week. A few days afterwards he got a stress when lifting a heavy mass of dough, and he felt at the time as if something had given way in his scrotum. Soon after he discovered a swelling in the spermatic cord above the left testicle; it was accompanied by some little feeling of pain and uneasiness, and, being of a nervous disposition, he got much alarmed, and forthwith consulted a medical man, who advised him to wear a suspensory bandage and to take perchloride of iron internally. He wore the bandage for two years, but without benefit, the pain still continuing, especially when he was overworked, when the swelling also underwent a marked increase in size.

On examination, the spermatic veins of the left side were found to be enlarged, being about the size of a large forefinger. The swelling was soft and doughy to the touch, was most marked at the upper part of the testicle, and descended slightly behind it. On tracing the veins upwards, they gradually decreased in bulk, and near the external abdominal ring they were of normal size; about an inch from this aperture there was another soft, compressible tumour, apparently due to a varicose condition of a vein or veins at this point. The testicle on the affected side was soft and somewhat atrophied. The right cord and testicle were healthy. There was a slight tendency to varix of the left external saphenous vein.

The patient was in a state of extreme nervousnese and depression, and was anxious to be operated on, as he had allowed his mind to dwell on his disease to such a degree that his health had given way, and he felt unfitted for mingling in or enjoying any society.

On Feb. 2nd the affected veins were ligatured subcutaneously in the manner suggested by Ricord, but silver wires were employed in place of silk ligatures. The free ends of the wires were twisted round two small pieces of catheter. With the exception of retention of urine during the first three days, and slight swelling of the epididymis, which was unaccompanied by pain, there were no symptoms worthy of notice. On the sixth day after operation the patient was attacked by diarrhœa, to which he had before been subject, which resisted treatment for several days, and gave rise to considerable increase of temperature, 102·8° F. being registered on Feb. 8th. After various remedies had been tried, a pill containing sulphate of copper and opium completely arrested it.

The ends of the wire were tightened from time to time, but

on Feb. 10th one of them unfortunately gave way, and after this another broke off. Fresh ends were with some difficulty applied, in the hope that the ring enclosing the veins might eat its way through them; but this it failed to do, and after waiting for nearly three weeks from the date of operation, and having consulted my colleagues, Professor Pirrie and Dr. Ogston, it was decided to cut off the remaining end, to allow the punctures to heal, and the ring to remain *in situ*. When the patient was dismissed a few days later a hard knot could be felt at and below the point where the ligature rested; above this the veins were not perceptible. I saw the patient about six months afterwards, and so improved was he in appearance that I scarcely recognised him. The pain had entirely disappeared, and with it his miserable forebodings, and he expressed himself as feeling as well as he could desire.

Two cases similar to that just narrated have been placed on record—the first by Mr. Redfern Davies, the second by Mr. Barnard Holt. In both the wires failed to effect division of the veins, and in both they were allowed to remain. Mr. Davies saw his patient a year afterwards, and found that all things remained as when he left. The patient had resumed work immediately upon leaving, and he was totally unaware of there being any wires in his scrotum. So well satisfied was Mr. Holt with the result in his case that "he considered it was a practice that might be repeated with advantage. Should future cases be attended with a like favourable issue, the proceeding would become general, and the pain and time of the patients be considerably abridged, while the question of recurrence would be entirely set at rest, such an occurrence being impossible so long as the wires acted as compressors." It does not seem to me, however, that this mode of cure would ever find favour either with surgeons or patients, when it can be achieved with almost equal certainty by mere temporary compression of the affected part.

The procedures suggested by Vidal de Cassis, Lee, Curling, Erichsen, Barwell, Bumstead, and Packard are all reliable, but, for my own part, I give preference to that of Ricord.

The substitution of wire for the silken threads which were formerly used is certainly a decided improvement as regards the risk of inflammatory irritation being set up; but the gain is not unalloyed, for, as is shown by the cases referred to, the wire, on account of the tolerance exhibited by living tissues to metallic bodies, may fail to effect division of the parts round which it is applied. Permanent obliteration of the venous channels is alone required, but, from the mode in which the loops are generally applied, they can only be got rid of when they have cut their way through the mass of veins they sur-

round. Mr. Tufnell, of Dublin, seeing the advantages that would accrue were the wires removable at pleasure, proposed the addition of two retractors—i.e., two loops of wire,—one of which was to be passed through each of the loops by which the veins are encircled before the latter were pulled within the scrotum. The suggestion is undoubtedly an ingenious one, but it occurred to me that the matter might be simplified by utilising the original loops, and so rendering further additions unnecessary. What I propose may, I think, be readily understood by a glance at the accompanying figures, for which I am indebted to my friend Mr. Arthur Clyne.

Instead of employing a needle with an eye near the point, a common surgical one, with a broadish point, should be selected. After threading it with a long loop of silver wire, the wire is to be knotted at a point distant about two inches from the eye of

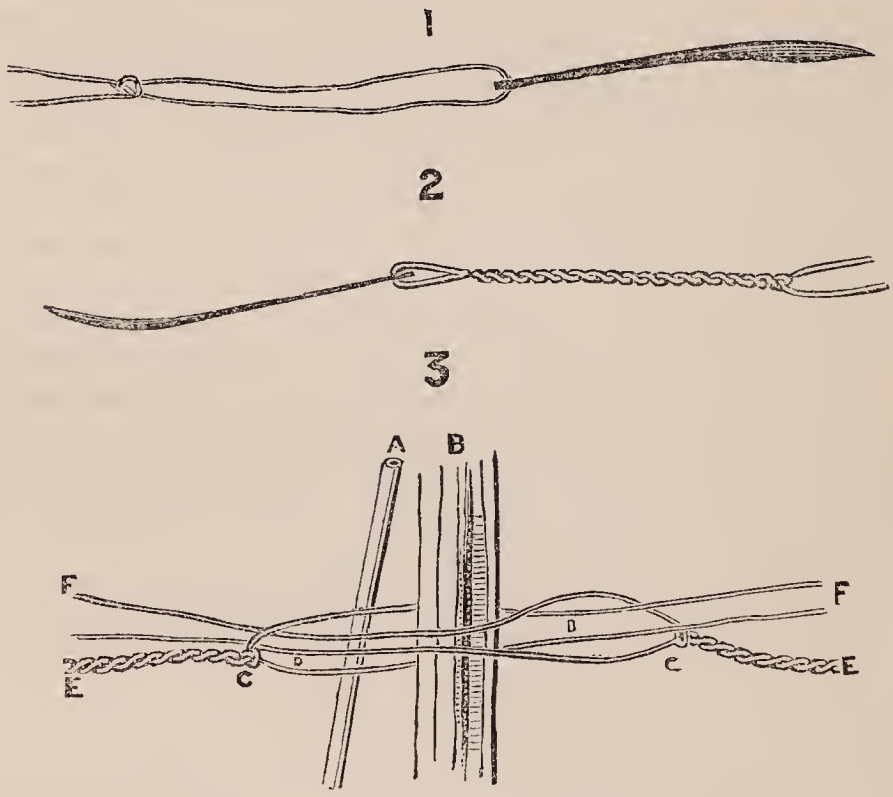


Fig. 1: Shows point where the wire should be knotted.

Fig. 2: Needle completely armed.

Fig. 3: Wires passed, but not tightened. A, Vas deferens; B, Vein; C C, Knots; D D, Loops; E E, Retractors; F F, Free ends.

Note.—In Figs. 1 and 2 the ends of the wires are represented as too short, and in Fig. 3 the retractors are also too short; this was necessary on account of the size of the columns by which the artist had to be guided.

the needle, and the wire firmly twisted from this knot to the eye. The vas deferens having been separated from the mass of veins, the needle is to be passed through the scrotal wall,

between the vas and the veins, and brought out at the other side. The wire must be drawn out so far as to leave the point at which it was knotted outside the scrotum. The needle may now be removed by slitting the wire at the eye. A blunt-pointed needle or probe, armed exactly as the needle was, is then to be inserted at the aperture of exit of the former one, passed over the opposite side of the veins, and brought out at the original point of entrance of the first needle, and the wire pulled out, and the probe removed, as before. The veins now lie between two double wires, the one in front, the other behind. There are two openings in the scrotum, through each of which there project two free ends, and a twisted portion of wire which has a loop directly in front of the point where the wire was knotted. The ends of each wire are then to be passed through the corresponding loop, and pulled tightly and forcibly in opposite directions until the vessels are completely strangulated. Compression should be kept up by fastening the ends around some resisting body, such as Mr. Tufnell suggested—viz., "a piece of watch-spring bent into an arc, the effort of which to restore itself keeps up continuous tension upon the cord." The twisted portions project on either side, and are thus available as retractors. When compression has been kept up for a sufficient length of time, the veins may be readily freed from pressure by snipping off the free ends, and making traction on the twisted ends. On August 4th I had the pleasure of assisting my friend Dr. F. F. M. Moir, of this city, in a case where he put this method into practice, and the result was eminently satisfactory, the removal of the wires being effected with perfect ease.

One little matter of detail is worth remembering when performing Ricord's operation—viz., that, instead of threading the second wire on a sharp-pointed needle, a probe, or other blunt instrument, should be used; for, as it has to pass through apertures already made, it does not require a cutting point, and by using a blunt instrument there is no chance of wounding any of the contents of the scrotum, and it ensures the second instrument making its exit at the exact point of entrance of the former one, so that the possibility of including any portion of the structure forming the scrotal wall is avoided.

The simplicity and certainty of the method suggested will, I trust, commend it to those who are called on to treat cases of varicocele by operation, as no appliances are required beyond those found in the pocket-case of every practitioner, and as compression judiciously applied in the manner indicated cannot possibly fail to secure obliteration of the affected veins.—*Lancet, Feb.* 24, 1877, *p.* 267.

74.—TREATMENT OF VARICOCELE.

By Dr. GEORGE H. B. MACLEOD, Surgeon and Lecturer on Clinical Surgery, Western Infirmary, Glasgow.

In the Lancet there is a very interesting paper by Dr. Will, of Aberdeen, on a method he employs for constricting the vessels in varicocele. That plan is very similar to one I have been in the habit of employing in public and private practice for some years, and which I last applied on the 19th of this month to a patient now lying in the Western Infirmary. I employ a small trocar and cannula to traverse the tissues behind and in front of the veins, and I pass a loop of strong wire through the cannula, one behind and another in front of the vessels, taking care to place these loops in opposite directions. I find the trocar and cannula a very speedy and easily guided means of placing the wires in the desired position. I introduce two sets or pairs of wires, the one below the other—*i.e.*, nearer the scrotum—and they are tightened, as described by Dr. Will, by passing the free ends through the loops, and when drawn tight the loops disappear within the tissues. The constriction is perfect, and has always, in my experience, performed the part required. I have never had to abandon the wires, nor have they ever failed to divide the veins. If it is thought right, the vessels can be divided subcutaneously with the tenotomy knife between the constricted points. Before drawing the loops home, I place a fine hempen ligature through each loop, so as to enable me to remove the loops at any time if I so desire. I think, however, that the presence of these ligatures is beneficial by augmenting the effusion we desire, and for the same reason such ligatures have appeared to me preferable to wire used for the same purpose. There is not, however, any real need of such ligatures for the removal of the wire loops, as, so soon as the veins are divided, the loops become loose, and can be withdrawn to either side. I fix the wires by placing a cork on either side of the scrotum, between the free ends of both sets of wires, and twisting the wires over it. The wires can be easily tightened at any time by drawing them firmer over the corks.

The plan of operating above referred to will be found very simple, and as effectual as any other.—*Lancet, March* 3, 1877, *p.* 332.

AFFECTIONS OF THE EYE AND EAR.

75.—CLINICAL LECTURE ON A CASE OF SQUINT.

By Dr. CHARLES BELL TAYLOR, Honorary Surgeon to the Nottingham and Midland Eye Infirmary.

It is well that we should have some means of expressing the extent of the deviation in cases of squint, and this is usually

done by measuring from the median plane of the orbit. If I make a dot in the lower lid exactly in the centre, which is the normal position of the pupil when the eye is at rest, and another in continuation with a vertical line drawn through the centre of the right cornea in its abnormal position, we find the distance between the two dots is exactly three lines and a half; and this is the extent of the squint. If I now close the left eye, and direct the patient to look straight with the squinting one, it assumes its normal position; but on raising my hand we find that the left eye has become the squinting one, and, moreover, that the extent of the deviation is exactly the same as that of the right eye. You will see this perfectly if, instead of closing the left eye, I hold a piece of ground glass before it. This shuts out the left eye from any participation in the act of vision, although you are able to see through the glass the exact position assumed by it when the right or squinting eye is looking straight. It is as though the two eyes were connected by a cord that was too short to permit them both to look straight together, and that whatever was taken on one side had to be given from the other. This is what is meant by "concomitant" —that is, the straightening of the squinting eye is accompanied by the squinting of the other; and the fact that the patient squints first with one eye, and then with the other, also brings the case under the definition of "alternating." The deviation of the right or squinting eye is called the "primary deviation;" the deviation of the left or sound eye, when covered or seen through the ground glass, the "secondary deviation." The primary and secondary deviations always correspond exactly in cases of concomitant squint; and it is essential to note this, because when the squint is due to paralysis the secondary deviation greatly exceeds the primary.

Convergent strabismus from paralysis is not by any means a common affection, but through the kindness of a private patient I am enabled to show you a case. This gentleman, Mr. P., from Derby, is thirty-six years of age, and, as you perceive, squints very decidedly inwards with the left eye. Now, if I direct him to follow my finger with the squinting eye, you see that he cannot force it outwards beyond the centre of the orbit, while the great effort he makes in this direction causes him to squint much more with the right eye than he did before with the left. To appreciate the value of this symptom you must remember that you cannot turn the left eye outwards without at the same time turning the right eye inwards—they are associated actions. The central nerve-ganglia of the left external rectus and the right internal rectus are intimately connected; and when this patient makes an effort to turn his paralysed eye outwards, although he cannot effect his

object, he is at the same time doing his utmost to turn the right eye inwards: hence the effect, although *nil*, or nearly so, on the paralysed external rectus of the left eye, is greatly in excess on the healthy internal rectus of the right. This distinguishes cases of paralysis from cases of concomitant squint; and you will notice a similar association of symptoms in patients suffering from uncomplicated paralysis of the superior rectus, where, owing to the connexion of the central nerve-ganglia of the superior rectus and levator palpebræ, the effort to look upwards raises the lid of the paralysed eye out of all proportion to its fellow.

Why should so many children begin to squint when they first begin to read, or write, or sew? Simply because they cannot see unless they do so. Their eyes are too short in the long axis—that is, from before, backwards. The refraction is too low, and objects, instead of being focussed upon the retina, are depicted upon a spot a little beyond it: hence the patient makes extraordinary efforts to lengthen his eye, or increase its refraction, so as to bring the picture into its proper place. You will understand this at once if you compare the accompanying outline of the short eyeball (Fig. 1) with its fellow of the normal eye. (Fig. 2.)

Fig. 1.

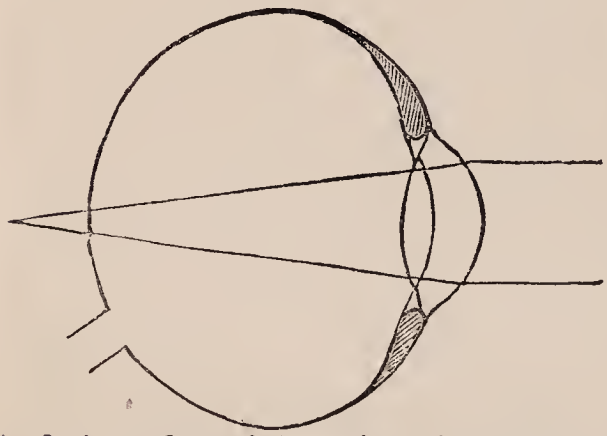
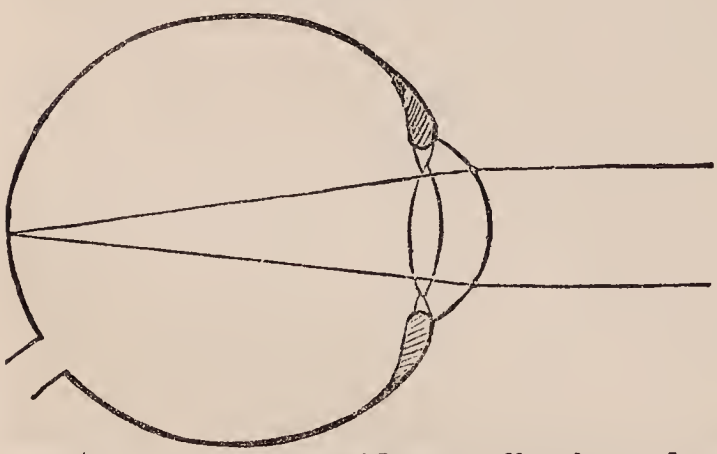


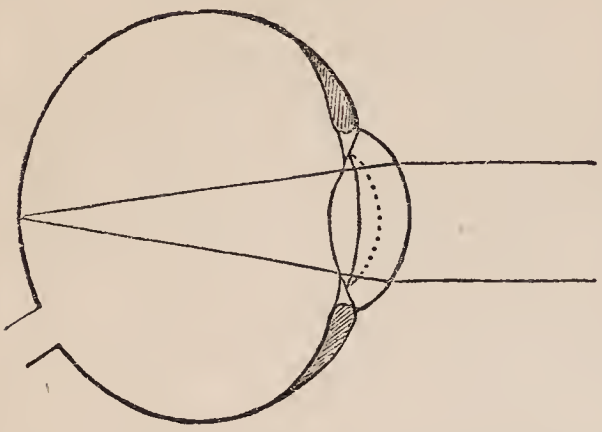
Fig. 2.



You may say, "What has this to do with squint? Squinting will not alter the shape of the eyeball." No; but it has a similar effect, as I will now explain. Patients whose eyeballs are too short are said to suffer from hypermetropia; they are obliged to exert a certain amount of accommodation—that is, render the lens optically stronger by increasing the curvature of its anterior surface by the active exercise of the ciliary muscle, even when looking at distant objects: an effort that is not at all required of the normal eye. Rays of light pro-

ceeding from objects twenty feet distant are parallel, or nearly so, and, in an eyeball of ordinary dimensions, are brought to a focus on the retina by the power of refraction of the cornea and lens alone, when the eye is at perfect rest, and without any exertion of the ciliary muscle (see Fig. 2). In hypermetropic persons it is necessary that the convexity of the lens should be increased before these parallel rays can be brought to a point, as indicated by the dotted line in Fig. 3, where the hypermetropic eyeball is seen successfully bringing parallel rays of light to a focus on the retina by increasing the curvature of the anterior surface of the lens—an effort which, in an eye of ordinary shape, is only required for the divergent rays which emanate from near objects. Now, if the hypermetrope is obliged to accommodate for distant objects, the rays from which are parallel, you can readily understand that considerable exertion is required in order to enable him to focus near objects, the rays from which are divergent; and this excessive effort of accommodation involves a squint, because the power of accommodation is greatly increased by convergence of the optic axes. The central nerve-ganglia of accommodation and convergence are intimately connected; they are associated actions—go hand in hand; and the one is necessarily accompanied by the other. If I put on a pair of concave spectacles, rays from distant objects are rendered to me divergent; I am compelled to accommodate for them, and I convert my normal eye into a hypermetropic one. If I now try to read, a still greater effort of accommodation is required, and I am at first unable to do so; but, with an extra effort, I manage to decipher newspaper type. Now, if you watch, you will find that this effort of increased accommodation has caused me to squint: that is, in order to exert sufficient accommodation to overcome this concave glass when looking at near objects, I am obliged to squint; and this is the case with hypermetropic children. If I use a strong pair of concave glasses, no effort at accommodation will enable me to overcome them; the squint in that case would be of no use, and is therefore not called into requisition. Hence, persons whose eyeballs are very short, who suffer from high degrees of hypermetropia, are not nearly so likely to squint as those suffering from moderate degrees of the same affection. You will often find whole families of

Fig. 3.



children who (paradoxical as it may appear) inherit squint from parents whose eyes are perfectly straight—the father suffering from a high degree of hypermetropia, while the mother's eyes are normal; or *vice versa:* the result being moderate degrees of hypermetropia in the children, who squint in order to overcome it, and are often supposed to catch the affection from one another. If I neutralise the concave glass with an equally strong convex one, no effort is required, and I can read as well as ever.

Why, therefore, do we not cure squint by neutralising the hypermetropia with a suitable convex glass? A most rational suggestion; and no doubt great good may be effected in this way, as well as by paralysing the accommodation with atropine; but, in point of fact, these patients do not come to us, as a rule, until they have squinted for some time. Often they only do so because their lives have been rendered miserable by the animadversions of their schoolfellows and playmates. Prolonged treatment with spectacles constantly worn, and which only mitigate the defect, still subjects them to remark—indeed, tends to aggravate rather than diminish this serious evil,—and, consequently, offers no attraction either to our patients or their friends. Moreover, it is dangerous for little children to wear spectacles out of doors, or when at play, they are so apt to be injured by fracture of the glass; and, strange as it may appear, after they have squinted for some time they won't leave off, even though you give them glasses so strong that the squint, far from assisting, actually disturbs vision. The old axiom, "*Sublatâ causâ, tollitur effectus*," does not apply here, for we are compelled first of all to remove the effect by operation, and afterwards to deal with the cause as best we may. We have therefore no option but to operate in this case; and in doing so we have choice of four methods—von Graefe's, the Moorfields, Liebreich's, and a modification of these which you have seen me adopt for some years past in the cases treated at this institution.

I pinch up with forceps an ample fold of conjunctiva and sub-conjunctival tissue immediately over the insertion of the internal rectus tendon, as in von Graefe's operation; divide this, and separate the conjunctiva pretty freely from the subjacent parts, so as to avoid shrinking of the caruncle (as suggested by M. Liebreich); pass a small hook along the under border of the tendon, and cause its extremity to project beyond the upper border. I then cut upon the point of the hook through the stretched conjunctiva with scissors, thus making a counter-puncture, as is Mr. Bowman's practice, and divide the remains of the tendon under the strip of conjunctiva which is allowed to remain.

Shall we operate upon one or both eyes? We have seen that

the eye which was closed, whether right or left, became for the nonce the squinting one. Now, if both eyes are closed, as in sleep or during anæsthesia, both squint; and the primary deviation, limited during waking moments to one eye, becomes equally divided between the two. Here is clear evidence of the binocular nature of the affection; and you can readily understand that it would be futile to operate upon one eye only for a squint which so evidently affects both. The tendon of the squinting eye should be divided rather more freely than the other; and in apportioning the dose between the two eyes it is well to test the sight immediately before operation, reserving the major procedure for the weaker eye of the two. Most authors recommend us to do one eye only at a time, and this would be, no doubt, a safe procedure if we could do as we

Fig. 4.—Original Squint.

pleased with our patients; but, in point of fact, we cannot do as we please, and in deciding upon the course to adopt we must take into account various outside factors which are likely other-

wise to disturb the result of our calculations. Two or three operations involve the repeated administration of anæsthetics, which patients dread more and more each time they are used. If I operate upon one eye only, the other eye is certain to squint, just as it does if closed or covered with ground glass; and, indeed, the operated eye itself will often in such cases look as if it wanted something more doing at it. When this is the case

FIG. 5.—Transferred to sound eye when the squinting eye was made to look straight.

the parents are disappointed, while friends and acquaintances who deprecated the operation are triumphant, and often succeed in persuading them to decline any further interference.

In my own practice I generally divide both tendons, and am not satisfied until parallelism is pretty nearly restored.

Here is a patient operated upon a fortnight ago, who has been good enough to sit for her photograph in order that you might compare her then and present condition. The original squint in this case was in the right eye, as shown in Fig. 4, trans-

ferred, when the squinting eye was covered with ground glass, to the left eye, as shown in Fig. 5. In this case I operated upon both eyes at once, securing at one *séance* the result shown in Fig. 6.

If I had operated on the squinting eye only in the first instance, the patient would have squinted with the other, and presented an appearance similar to that shown in Fig. 5, and she would have returned home—a considerable distance—very little better for what had been done. Of course such a condition would admit of remedy; but, as I remarked before, you cannot

Fig. 6.—Result after operation.

do as you please with your patients, and if you send them away only half done, as it were, it often happens that they do not return. If I have reason to suspect that too much has been done, I limit the effect by catching up the tendon and adjacent tissues with a suture, and stitching it to the conjunctiva and subconjunctival tissues at the inner edge of the cornea, taking

up a greater or less portion of tissue, according to the effect it is desirable to produce. When the patient has completely recovered from the effects of the anæsthetic, this may be tested, and the suture loosened, tightened, or removed, at pleasure. The effect of the operation is simply to permit the tendon of the internal rectus muscle to fall back and become attached to the globe lower down. The first effect is somewhat diminished when the tendon becomes re-attached to the globe, and is afterwards, in the course of a few weeks or months, slightly increased, owing to the action of the opponent muscle, which is now enabled to exert a greater influence upon the position of the eye, so that a slight residual convergence will often disappear.

This desirable end is much more certainly attained by the use of spectacles which correct the hypermetropia. If, on the other hand, we find that the ultimate result is a tendency to eversion, the malposition may be corrected by withholding glasses, so that the necessity for convergence accompanying increased accommodation may help to restore parallelism of the globes. If, in spite of this, we find that one or both eyes are turned slightly outwards, they may readily be put straight by dividing both external recti muscles. Practice and attention to the rules I have laid down will enable you to command the most brilliant results in cases of squint. We have operated upon thousands of patients at this institution. It is a rule that each shall return within a few months, so that we may note the ultimate result. One easily forgets faces in a large practice; and it frequently happens, as you know, that we fail to recognise them, so completely has the ocular defect been removed.—*Medical Times and Gazette, March* 17, 1877, *p.* 279.

76.—A NEW EYE DOUCHE.

[Mr. Priestley Smith, Ophthalmic Surgeon to the Queen's Hospital, Birmingham, in a note to the Editor of the Lancet says:]

Several patients to whom I have recommended the use of an eye-douche having complained to me of the unsatisfactory working of the instruments which they obtained, I applied some months ago to Messrs. Arnold and Sons, of West Smithfield, to provide me with a better. They have made for me a new eye-douche acting upon the principle of their "simple enema" syringe, as shown in above engraving. It has worked extremely well in my hands at repeated trials, and it seems little likely to get out of order. I beg leave therefore to bring

it before the notice of the profession, and to recommend it as a well-made and useful instrument. (The eye should have beenrepresented closed instead of open.)—*Lancet, Feb.* 17, 1877, *p.* 255.

77.—SELF-RETAINING EAR-SPECULUM.

Mr. J. Millikin, of St. Thomas's-street, Borough, has just brought out a somewhat ingenious and yet simple self-retaining ear-speculum. In form it is like the ordinary funnel-shaped speculum (Grüber's); but it is divided down the centre into equal halves (see diagram), which are connected by a piece of watch-spring. By this means its tubular extremity opens and closes, and thus adapts itself to any sized meatus. This is an obvious advantage, as it does away with the necessity of holding the speculum in position: thus the surgeon has both his hands free, and he can do what he has to do without loss of time, or the services of an assistant. Another advantage is that a single speculum is all a surgeon requires; it fits any sized ear. We would suggest as an improvement on this useful little instrument that the spring be silvered, in order to prevent its becoming rusty; and that the speculum be put into a nice portable little case. We doubt not that it will be extensively adopted.—*Medical Times and Gazette, Jan.* 6, 1877, *p.* 25.

SYPHILITIC AFFECTIONS.

78.—ON VACCINO-SYPHILIS.

By James R. Lane, Esq., Surgeon to St. Mary's Hospital.

Lancereaux collected 351 cases in which vaccination was performed with lymph which appears to have been impregnated with syphilis. Of the 351, 258 contracted syphilis, while 93 only escaped.

Since Lancereaux's work was published, we are indebted to Mr. Hutchinson for his series of cases, which are recorded by him in his usual careful and complete manner. In the first series in 1871, twelve persons, mostly young adults, were vaccinated from an apparently healthy child, but which soon afterwards showed evident signs of constitutional syphilis. In all twelve the vaccination progressed satisfactorily, but in ten indurated chancres were developed at the seat of the punctures, by the eighth week. All were treated with mercury, and in all the primary sores quickly disappeared, but in four of them constitutional symptoms appeared five months after the vaccination. There is reason to believe that blood was mixed with the lymph in these cases. Mr. Hutchinson's second series occurred in May, 1871. The vaccinifer was a child in good health and condition, but with slight local symptoms of inherited syphilis. Twenty-six persons had been vaccinated at the same time from this child. Out of the twenty-six there were unmistakable symptoms of secondary syphilis in nine, all children, suspicious symptoms in six others, and entire escape, as far as was known, of the remainder. Mr. Hutchinson published two other cases, which I need not detail, but will merely observe that in them, as in the second series mentioned, there was no evidence that the lymph was contaminated with blood.

It seems clear, then, that cow-pox and syphilis can be conveyed simultaneously by inoculation. When this takes place, the two diseases are developed independently and without interfering the one with other, and there is a singular uniformity in the course which they are found to follow. The vaccine vesicles are developed as usual, but at the end of a month or five weeks, and usually not till after they have healed, a chancrous induration appears at one or more of the inoculated points, and a few weeks later the ordinary train of secondary affections follows. Or the result, as regards vaccination, may be negative, as it so often is in revaccination; the punctures heal and are forgotten, but after the usual interval the syphilitic symptoms make their appearance.

There were some who blamed Mr. Hutchinson at the time

for making these cases public, but he wisely deemed it better that the knowledge of such a danger should be diffused as promptly and widely as possible. The profession in England had till then been incredulous on the point, and a full knowledge of the risk was clearly the first step towards preventing the occurrence of such lamentable accidents in the future.

I have referred to the prevalent opinion that it is the blood, and not the vaccine lymph, which conveys the contagion—a view which seems to be strengthened by the fact that Rollet and others have vaccinated with lymph from syphilitic children, carefully avoiding any admixture of blood, and the result, as regards syphilis, has hitherto been negative. Such negative results, however, are quite inconclusive; for, even with syphilitic blood, carefully inoculated on purpose, the failures have been in the proportion of about three in every four attempts. The question is,—Can we be certain that the admixture of blood is necessary? Mr. Hutchinson thinks that probably it is not, but he evidently thinks that it greatly increases the risk, for he suggests that if the vaccine vesicle be drained too freely, corpuscular elements of the blood and tissues may readily become mixed with the lymph, and that this mixture may account for some persons becoming infected while others vaccinated from the same source escape. This, however, is hardly more than an hypothesis, and the only safeguard we have is to inquire carefully into the antecedents and present condition of every child before vaccinating from it, and if there is the slightest doubt attaching to its history, to reject it as ineligible. It is clearly proved that syphilitic blood may convey syphilis, but it is not yet proved that vaccine lymph from a syphilitic child, however carefully taken, will not, and it would therefore be the height of imprudence to act on any such belief.

Mr. Hutchinson's cases are all examples of direct communication from a diseased child to those vaccinated from it, and this is a mode of transfer which one's mind readily admits; but it is very difficult to conceive that healthy persons so inoculated, when made use of a week afterwards to vaccinate others, and long before they have themselves developed any syphilitic symptoms, should be equally capable of conveying that disease, as is stated to have happened in most of the continental groups of cases. Can it be possible that a syphilitic inoculation which does not affect the system sufficiently to produce even a local result in the form of a primary sore for five or six weeks, and no general result till some weeks later, should in one week have so contaminated the whole mass of the blood as to render it, or its products, inoculable at the end of that short time? This would imply a general and thorough infection of the system in the early part of the incubation period—a view in the pathology

of syphilis which, as far as I know, has never yet been suggested.

It is so extremely unlikely that, for myself, I cannot accept the facts as authentic without much fuller and more rigorous investigation. If it were true the prospect would be indeed alarming. It would be useless to inquire into the health of a child before vaccinating from it, for it might have been infected by vaccination from a syphilitic child a week before, in which case it would appear perfectly healthy, and would not present any outward sign of syphilis till four or five weeks later. It would, in fact, be necessary to search back through a series of five or six consecutive vaccinations before any certainty of safety could be attained. If the disease were really capable of being conveyed in this manner in geometrical progression, vaccine syphilis would surely have been the rule amongst us by this time, instead of being the very rare exception, which we know it so fortunately is. There would then be nothing for it but constant recurrence to the cow — an expedient which might probably be resorted to with advantage much more frequently than it is.

Contagion of the Secretions of Syphilitic Patients.—The question of the contagious property of the secretions of syphilitic persons, such as the pus of an ordinary abscess, the morbid secretions of mucous membranes, the pustules of acne, the serum of eczema, &c., is analogous to that of unmixed vaccine lymph. The matter of acne and of eczema from syphilitic patients has been inoculated by Diday on healthy persons, but with no effect, and the tendency has been to consider all such secretions as harmless, or, at least, that the contrary is not proven. Lately, however, in 1870, the late Mr. Morgan, then surgeon to the Westmoreland Lock Hospital, Dublin, related some experiments which favour the opposite conclusion. He performed a number of inoculations with the matter of purulent vaginal discharge. When the discharge proceeded from a patient free from syphilitic taint, the result was negative, but when it was taken from a patient with secondary disease, the effect was the production of an ulcer, exactly resembling a soft chancre, and, like it, readily re-inoculable. He was careful to ascertain by examination, externally and through the speculum, that no ulceration, either primary or secondary, coexisted with the discharge. His experiments were performed either on the patients themselves or on others who were syphilitic; he did not, of course, feel justified in inoculating healthy persons. The result was exactly what has been found to happen when the matter of hard sores or of secondary affections has been inoculated on syphilitic patients, and there is good reason to suppose, therefore, that these purulent discharges from syphilitic patients are charged with syphilitic infection, and might

convey it to an untainted person. This would explain the result of Hunter's inoculation of himself, and also those exceptional cases in which syphilis is apparently the result of gonorrhœa.

Next, as to the physiological secretions of syphilitic persons—the tears, the saliva, the perspiration, the semen, and the milk. The balance of opinion is against their contagious quality, alway supposing them to be unmixed with the products of any secondary lesion. Inoculation with the first three—namely, saliva, tears, and perspiration—has been tried, and has hitherto failed. I do not know that the inoculation of semen has been tried, but I believe there is no instance in which, when the male organs were free from disease, the semen of a syphilitic patient has infected a female in the usual way by the production of an indurated sore. But there is another way in which the semen may possibly convey disease. There are some who think that an infected man, cohabiting habitually with the same woman, may devolop a secondary infection in her by the gradual absorption of the elements of the contaminated seminal secretion into her circulation, without the intervention of pregnancy, or any kind of primary lesion. The late Mr. Porter, of Dublin, and the late Mr. Langston Parker have recorded cases in favour of this view. Both are authorities whose opinion deserves attention, and the point is one well worth further consideration. The difficulty in all these cases is that the woman may at some time during the cohabitation have had an indurated sore, which caused her so little inconvenience as to remain unnoticed.

With respect to the milk, it is generally believed that a healthy child suckled by a diseased nurse will not contract disease unless the nurse has some secondary lesion on the nipple or in its neighbourhood;—that the milk itself, in fact, does not convey the disease. It is possible, however, that the milk, although diseased, may, if the child's mouth and lips are free from abrasion, be passed into the stomach without causing any mischief in its transit, and then be so changed by the action of the gastric fluid as to lose its contagious quality. Some experiments do, in fact, throw some doubt on the innocuity of this secretion. M. Voss, of St. Petersburgh, has recently inoculated three prostitutes with milk from a syphilitic woman. The milk was injected subcutaneously with a syringe. One of these women was already syphilitic; in her no result was obtained. In another, whose condition as to previous syphilis was doubtful, the result was also negative; but in the third, a girl aged sixteen, who had never had syphilis, papular spots appeared around the inoculated point forty days afterwards, and were soon followed by a general syphilitic eruption.

I do not pretend to offer a decided opinion on these various

uncertain points; but I see no reason why secretions, whether natural or morbid, which are formed from diseased blood, should not partake of the contagious quality of the fluid from which they are derived. At all events, the only safe course is to regard every one of them with profound distrust; and to take such precautionary measures, and to give such advice, as the circumstances seem to call for.—*Lancet, April* 21, 1877, *p.* 562.

AFFECTIONS OF THE SKIN.

79.—GLYCEROLE OF NITRATE OF BISMUTH IN SKIN DISEASES AND SOME OTHER CONDITIONS.

By Dr. BALMANNO SQUIRE, Surgeon to the British Hospital for Diseases of the Skin.

A short time since I described the preparation and detailed the advantages of a glycerole of the subacetate of lead. The result already has been that the preparation is now commonly used by practitioners throughout the country, not only for the treatment of various chronic diseases of the skin (more especially chronic eczema), but also as a useful means of employing lead topically in the treatment of diseases of the uterus.

The advantages of bismuth as a local application in certain conditions are equally appreciated, but the difficulty of employing it in a liquid form is a well-known obstacle to the development of its valuable properties. Like lead, it acts as a topical sedative and astringent. Its chief use is as a local application to the interior of the stomach, and accordingly it is largely prescribed as a sedative in cases of painful digestion and as an astringent in cases of water-brash. It is also, but less largely, used as a remedy in skin diseases; and it has been found serviceable as an injection in lingering gonorrhœa attended with exceptionally profuse purulent discharge. I was therefore desirous of having a glycerole of it if possible. Accordingly, I applied to one of the leading chemists in this city, and requested him to make me up a solution of the nitrate of bismuth in glycerine, to take away with me. I was assured by him that it was impossible, inasmuch as the salt would not dissolve in glycerine; and, as another difficulty, the nitrate of bismuth was never kept in stock,—there was no demand for it. I pressed him to procure some and to dissolve it for me. He promised to do so, but told me he knew perfectly well what would be the result. The next day I received from him a perfectly clear solution containing a drachm of nitrate of bismuth dissolved thoroughly in an ounce of glycerine. Now, this is in many ways, I venture to affirm, a valuable preparation—more valuable, perhaps, on

the whole than the glycerole of the subacetate of lead. Here we have a soluble preparation of the simple salt of bismuth; and a tolerably strong solution too. The liquor bismuthi et ammoniæ citratis of the Pharmacopœia is of course a *double* salt, and it is a question whether it has in full all of the valuable qualities as a local application of the *simple* salt of bismuth. On putting some of my glycerole on the tip of my tongue, I found it merely astringent, and slightly acidulous On rubbing it well into my tongue I found only the same effect, but more decided in degree. I was told that I should find the preparation very irritating, inasmuch as the salt comes under the chemical heading of "acid salts"; but I do not find it so even in the least degree. I thought I would just ascertain that my friend had really dissolved the quantity of bismuth he professed to have done, by availing myself of the simple test of pouring a little of it into a wineglassful of water, and stirring the mixture up with my finger to see how much white precipitate of trisnitrate of bismuth I should get. To my astonishment, I got no precipitate. So I poured in a little more; still no precipitate whatever. However, I left the glass to stand for an hour or two, and then looked at it again, and found a milkiness of the solution, and a fine milky deposit on the surface of the glass. I therefore arrived at two conclusions—the one is that nitrate of bismuth, which is so utterly insoluble in water, is perfectly soluble in glycerine; and futhermore that the presence of glycerine in water is capable of delaying, more or less, the precipitation of the nitrate of bismuth in it. So that where glycerine is a permissible vehicle we have now a ready means of *applying*, a simple solution of bismuth; and where glycerine is not permissible as a vehicle we have equally a ready means of *administering* a simple solution of bismuth in water, provided we do not mix it too long before it is required to be used.

As an application to the skin, or to the vagina or uterus, or to the throat, when bismuth in solution is required, nothing can be better than the solution of the nitrate of bismuth in glycerine. As a mixture to be taken internally, nothing more is necessary than that the mixture should be ordered as a solution of the salt in glycerine—one teaspoonful for a dose, to be mixed in half a wineglassful of water before taking it. Then, if the patient does not look too long at it before he swallows it, he will certainly swallow a perfectly clear solution, and one, moreover, which will probably do him more good than any other preparation of bismuth that has yet been devised.

The difficulties presented by nitrate of bismuth are well described in Fownes' "Manual of Chemistry." It was "the basic property of water and the feeble affinity of teroxide of bismuth for acids" that I sought to elude by employing glycerine as a sol-

vent in place of water, if only I should haply chance to find that glycerine would prove itself to be a solvent in this case; and the result was that I succeeded quite to my content. I relied, and not unduly so, on the remarkably solvent powers of glycerine, and on its strong affinity for water. It will be so obvious as scarcely to need stating that a soluble preparation of any drug is far more active and efficient than an insoluble one, and that the simpler *that* preparation may be, the more are we likely to get the unmodified and full effect of that drug. In all cases where an astringent yet unirritating application is required, this preparation cannot fail to be of signal service. In diseases of the stomach its action cannot fail to be more prompt and more certain than any preparation of bismuth that has yet been used.

As an application for diseases of the skin a field is opened out for the employment of bismuth with an amount of efficiency and precision that has never yet been possible; and the variety of diseases of the skin in which even the comparatively inert, because insoluble, trisnitrate has been found of service are too numerous to refer to in this place. In diseases of the female genital organs, accompanied with tenderness and profuse discharge (a sufficiently common class of cases), we have here a remedy which requires only to be carefully tested to insure, most probably, very excellent results. In inflamed sore throats its value as a paint or as a gargle will most likely be very considerable; and as an injection in gonorrhœa, after the acute stage has passed over, or in cases of chronic gleet, attended with an irritable state of the urethra, it cannot but prove of signal service. It is for those who have devoted themselves particularly to the study of these various classes of disorders, and of the effects of remedies on them, to say whether I am right or not in believing that I have stumbled on a useful addition to our means of treating them.

It may be necessary to add here that a much smaller proportion of the soluble nitrate of bismuth ought to be employed than is customary in the case of the insoluble trisnitrate (or subnitrate), and *that* equally, whether the amount of a dose for internal use may be under consideration, or the strength of a solution for external application.

In comparing a glycerole of nitrate of bismuth as against a glycerole of subacetate of lead, I find that, in cases of chronic eczema, the former (equal strengths of either being employed) has a much more active and stimulating effect than the latter; but then it must be remembered that the former is a simple salt, while the latter is a basic one. Unhappily we are unable as yet to obtain a solution of a *basic* salt of bismuth in a neutral menstruum. The dose for internal use of the simple nitrate of bismuth must therefore be a moderate one, or at least the dose

should be largely diluted, and the strength of the glycerole, as an outward application should in most cases not exceed ten grains of the nitrate to the ounce of glycerine.—*Medical Times and Gazette, Dec.* 9, 1876, *p.* 646.

80.—TREATMENT OF SCROFULOUS LYMPHATIC GLANDS BY A PAINLESS ELECTROLYTIC CAUSTIC.

By Dr. C. H. G. Bird, Assistant-Surgeon to Guy's Hospital.

Before describing this treatment the class of cases in which it has been employed, and for which it seems especially adapted, must be defined. From first to last the morbid condition of the lymphatic glands known as "scrofulous" or "strumous" passes through three very distinct stages, which it is necessary to distinguish, in order to classify the methods of surgical treatment suitable to each case.

In the first stage the glands increase in size, with pain amounting to little more than inconvenience, and with a complete absence of those very acute inflammatory symptoms that accompany their rapid enlargement, which results from the absorption of some irritating material from an adjacent suppurating surface, and which enlargement ends usually in acute abscess. This is not the course of abscesses that result from scrofulous inflammation of the lymphatic glands. To some source of irritation, indeed, the disease may often be traced; and though this may eventually disappear, yet the enlargement continues for months and even years. In this stage the surgeon does not interfere; he trusts to general improvement of the health and to better hygienic conditions to stay the progress of the disease. After a while all may subside, or at once the third stage may supervene; if not, the second stage will be reached, where, active growth ceasing, the swollen and hard glands remain as tumours beneath the unbroken skin, and can be distinguished easily on manipulation; or the whole mass having become agglutinated and encapsuled, a lympho-sarcoma is the result.

Though caustics may be employed for the removal of these gland-masses, yet, especially where they are freely movable or encapsuled, excision is the best treatment. One of the cases I shall narrate was in the second stage, and though a complete recovery followed in seven weeks, without any suffering, yet it would have been even more rapid had excision been adopted. The case was under Mr. Cooper Forster's care, by whose kind permission I am enabled to make use of it.

The characteristics of the third degree of the inflammatory state differ from those that precede it. The glands have now

mostly ceased to enlarge, unless they are doing so simply as the result of the irritation occasioned by the more active change going on in their neighbourhood; they are no longer free, but form a mass in which the outline of the original glands may perhaps still be distinguished; but whether this be the case or not, the whole is bound to the overlying skin and to the surrounding tissues by inflammatory adhesions. Hard in some parts, it is red, tender, and softening in others; and here fluctuation may be detected; and then, if the pus be evacuated and poultices applied, the opening, in lieu of contracting and healing after discharging a slough, often remains patent, and a granulating mass protrudes, which may exist, with little or no tendency to heal, for a long time. The treatment in this stage is not that by excision. The patients are generally sickly, and bad subjects for operative interference, even were it possible; but it is scarcely so in many cases, for the glands, now matted by inflammation, often enclose between them important structures. Where, however, it is feasible, the measure usually adopted is to incise the overlying tissues and then scoop out all that is removable with a blunt scalpel or spoon-handle. In by far the greater number of patients the only application is a poultice, or simple water-dressing, time being invoked to complete the cure.

Another alternative yet remains in the removal of the glands by caustics; and I shall only mention that by chloride of zinc, for the electrolytic caustic depends upon the formation of this salt for its destructive properties. Chloride of zinc is usually employed as points or arrows, the salt being made into a mass of sufficient consistency by its admixture with flour or gluten. A horny substance is the result, which, cut into wedge-shaped pieces, can be inserted into the part required, a puncture having been previously made with a bistoury. Nothing can be more desirable than the results obtained from this form of caustic. It acts at whatever depth it may be inserted; it destroys the tissues around it; and after some days the remnant of the "arrow" may be removed, and together with it a grey, hard slough; a cavity remains, that gradually fills up and cicatrises. It has, however, in common with all the ordinary caustics, certain serious drawbacks. The patient must be invalided during the treatment, and the pain is very severe during the first forty-eight hours; and this last, in children, becomes a serious impediment to the use of the caustic altogether, while unfortunately, it is in children that we mostly meet with the condition requiring the remedy.

By employing the chloride of zinc in the way I am about to describe as effective a caustic may be obtained as where the "arrows" are used, and one which is quite painless as a rule.

That the caustic should be painless at first sight seems strange; but when we have considered the exact mode of production of the chloride of zinc, taking into account the slight natural sensibility of the parts operated upon, the explanation of this circumstance will be easy. The whole treatment depends upon the production of the chloride of zinc, in the tissues of the body, at the expense of the chlorides in the serum, which are caused to unite with a slip of metallic zinc thrust in at the spot destined to be destroyed. To understand this, I need but recall what happens in a Smee's battery when the electrodes are united and the circuit consequently completed: not only is an electric current established, but the acid uniting with the zinc forms sulphate of zinc, while the hydrogen is evolved at the electro-negative or silver plate. If in lieu of sulphuric acid we employ chloride of sodium, a parallel change takes place: chloride of zinc is formed, and the sodium is set free at the silver plate, and becomes at once oxidised into soda. In this case, therefore, we have, instead of the two inert metals, two active salts; and if any albuminous substance, capable of being acted upon by the chloride of zinc, be present in the solution, it will be simultaneously affected. Let the silver and zinc plates be therefore placed one on either side of a piece of flesh, and be connected together by a copper wire, and the same changes will occur. The flesh is—for our purpose—a firm albuminous basis, bathed in serum or chloride of sodium solution; and the chloride of zinc that is formed, having a powerfully coagulating (and therefore destructive) effect upon albuminoid tissues, will cause marked changes in the space of a few hours. The flesh under the zinc plate will be no longer red, soft, and alkaline, but will be converted into a grey friable material, strongly acid to test-paper; while the presence of the soda under the silver plate may be proved by its strongly alkaline reaction. Each molecule of chloride of zinc, as soon as it is formed, or even in the very act of forming, exerts an influence upon the albumen with which it is in contact, and coagulates it; and it is to this action of the chloride of zinc in a "nascent state"—to use the chemist's phrase,—or, in other words, to its action by successive particles at the moment of their formation, that the absence of pain, when the living body is being operated upon, must be ascribed.

To carry out the plan of treatment of which the above is the theoretical explanation, the surgeon must be provided with some silver foil, at least the thickness of cartridge paper, some of the thinnest sheet zinc (that can be easily cut with ordinary scissors), and some fine copper wire. The zinc is cut into slips of the size required, according to the depth to be destroyed, pointed at one end, and pierced with a hole at the other. A slip, $1\frac{1}{4}$ in. long by $\frac{1}{3}$ in. wide, gradually tapering to a point, is a useful size. To extend its surface it may be bent into a

horseshoe, or, if required to destroy a long but narrow surface, the edge of a piece of zinc coarsely serrated may be employed. Whatever form be selected, let the piece of metal pass through a shield of thick gutta-percha or cork, both for the purpose of regulating its length, and to give attachment for the strapping by which it is fixed. The silver foil should be of the size of a shilling, or a little larger, and to the centre of one surface should one end of a short piece of copper wire be soldered; the other end of the wire is thrust through the hole in the free end of the zinc point, and twisted firmly to it. Thus a battery is constructed with its electrodes united and ready for action. Several pieces of zinc may be used in connexion with one piece of silver if necessary. Two raw surfaces must now be selected, one for the insertion of the zinc, the other for the silver. Of course the case, if a suitable one, will present at least one sinus for the zinc, and that for the silver can be easily made, and is best made by raising a small blister in the neighbourhood. When operating on the neck I usually blister over the mastoid bone.

Into the sinus that leads down to the gland-mass the zinc must be thrust until it is firmly embedded in the part that is to be destroyed. The required length can be easily measured beforehand, and the position of the shield regulated accordingly; and so it may be thrust with one stab up to the gutta-percha at once; it is then retained by strapping. The silver is simply laid on the other raw or blistered surface (the blister having been recently cut, so as to be moist with serum), and there strapped also *in situ*. A battery, with its circuit completed by the copper wire electrode, is thus formed, the exciting fluid for which is the serum in the patient's blood. One caution is neccessary—viz., let no solder get on that surface of the silver which is nearest the patient, or a sore will result. As a rule, the zinc is sharp enough to enter at once, if not, a bistoury puncture can be made. Sometimes a patient feels the prick of the zinc, but as often not, and then no further pain or inconvenience follows. Four or five days are usually enough for each application. The following changes will then be noted. The blistered surface will be found drier, strongly alkaline in reaction, and in part healed, for under the silver plate cicatrisation of a wound is promoted. On removing the zinc a gray slough will be seen, in lieu of the red granulation tissue before visible, and strongly acid to test-paper. At times this slough will come away with the piece of zinc, or it may be detached with forceps, or if not loose, it may be removed by poulticing. The apparatus can then be reapplied if necessary, for the same pieces of metal, if kept clean, may be used many times. (The apparatus is made by J. H. Sandy, Cooper's-road, Old Kent-road, S.E.)—*Lancet, April* 21, 1877, *p.* 564.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

81.—INSTRUMENTAL DELIVERY WITHOUT THE KNOWLEDGE OF THE PATIENT.

By Dr. James Braithwaite, Vice-President of the Obstetrical Society of London; Lecturer on Diseases of Women and Children at the Leeds School of Medicine; Assistant-Surgeon to the Hospital for Women and Children, Leeds.

There are many cases of lingering labour owing to failure of expulsive pains or to slight disproportion, in which the accoucheur hardly likes to use the forceps, although he well knows that they would at once relieve his patient from her suffering, and at the same time decrease the liability to still-birth. Ergot would answer the first of these desiderata, but not the second. There is, however, a sort of quiet opprobrium attending the use of the forceps; as the friends and neighbours discuss the propriety of the operation afterwards, often to the detriment of the man who really has simply done his duty. During the last twenty-two months, before which period I had not noted my midwifery cases, the forceps have been used in my practice 38 times in 384 deliveries; and 37 of the children were born alive, and all the mothers did well, no complications of any kind following, except in one case, *pelvic cellulitis*, which ended without suppuration. Some of these cases were very difficult, requiring the use of a powerful pair of long forceps; but to this class of case I make no reference in this paper, only to cases of slight disproportion, or of failure of expulsive pains when the head is low down in the pelvis. Looked at in their true light, however, some artificial assistance is really very important in these cases as well as in the others, to save the strength of the patient and the life of the child. As, however, human nature is after all only human nature, and we cannot always afford to go too strongly against public female opinion, I have for many years been in the habit of merely casually mentioning to the nurse or friend that I proposed to "give a little assistance" to the patient, but have not thought it always necessary to do so to the patient herself at the time—invariably, however, in that case mentioning it afterwards, before leaving the house or else the next day. When this plan is adopted the patient estimates at its really small importance the nature of the operation, and is generally very grateful that she had not been told of it at the time.

Now, however, I come to the point. With the ordinary forceps in any of its forms this cannot be done—but with the forceps which I described in 1869 (British Medical Journal, Dec. 25th, p. 673, and June, 1870, p. 581) it is perfectly easy; for they can be applied with but little manipulation, and without any disturbance of the position of the patient. The article to which I refer excited at the time so much interest, judging from the numerous letters I received, that I venture, after the lapse of seven years, again to draw attention to the subject—especially as I have in some slight particulars improved the instrument. Its peculiarity consists in both blades being introduced at once directly into the hollow of the sacrum. The blades and handles are accurately fitted to each other, the convexity of the inner blade to the concavity of the outer one, and this adaptation is maintained by a metallic clip on the handles. They are thus practically one blade during introduction, and before separation by removal of the clip. There is nothing peculiar in the blades when separated. If a pair of Simpson's short forceps is arranged with one blade inside the other, a very good idea of the instrument will be obtained. Although, however, I made Simpson's short forceps the model of mine, the blades of the latter are rather lighter and narrower, to lessen the weight and facilitate introduction. (The total weight with the clip is about 10½ ounces.) The introduction of the blades into the sacral hollow is not felt, because the external parts of the mother are not touched by them. Being small and narrow they are passed between the first two fingers of the left hand and the fœtal head. The clip is to be loosened a little before introduction, and after introduction is to be removed altogether; the blades are then to be glided round the head in opposite directions and locked in the ordinary way. I generally do this as each pain comes on, not during the complete absence of pain, nor during the intensity of one. When the blades are locked, the extremity of the upper blade, in the first or common position of the head, will lie on the right temporal ridge, just within the line of the hairy scalp; and the lower blade will lie behind the left ear; in the second position this will be reversed. The long forceps seize the fœtal head in the same manner, and to my mind it has some advantages over the side to side grip; these I stated in the original paper. When the head is emerging the blades should be unlocked and allowed to lie flat upon the head, when the handles will cross one another; or the instrument may be entirely removed, the expulsion being completed by the natural pains. This plan avoids the risk of rupture of the perineum. Three or four years after the publication of my previous paper on this subject, Dr. Vacher, of Liverpool, described a pair of forceps invented by himself, but

really only differing from mine in the handles being almost done away with, and what remains joined by a hinge. The blades are passed together, as in the way described, and then glided round the head in opposite directions. I cannot, with all deference to Dr. Vacher, see that there is much "principle" in joining the blades together by a hinge; the principle of the instrument is the introduction of both blades as practically one. I consequently think Dr. Vacher's instrument merely a modification of mine. When the blades are inseparably joined by a hinge and a catch they become merely tractors, and can have no power of compression. As a result of this the traction must be made by the extreme ends or tips of the blades which from the limited area of the pressure are more likely to injure the soft parts of the head. Moreover, the space between the blades when separated and in position on the head is always the same, whereas the size of the head varies a good deal. A tractor is useful in a more limited class of cases than an instrument by which a certain amount of compression can be produced, and which gives a pretty powerful grip of the head if necessary. I do not see the advantage of giving up the possession of a power which need not be exercised if not necessary, unless we get some advantage in another way by it. The forceps I use were made by Messrs. Joseph Gray & Co., of Sheffield; but they may also be obtained from Messrs. Joseph Wood & Co., of York, and Messrs. Weiss, of London. They are so small and light that they can be slipped into the coat-pocket and taken to every labour—a great advantage in country practice—and the accoucheur may have the comfort of knowing that they will do for pretty difficult cases of impaction if the head is low down in the pelvis, and he is too far from home to send conveniently for a larger and stronger pair. I will conclude with one case—the only one in which I tried delivery without saying anything to either friends or patient, and that in a difficult case—chosen simply to test the powers of the instrument. The pelvis was rather small, and the head had after many hours come well down, but then became impacted and the pains more feeble. One woman was leaning over the foot of the bed, close to the patient; another was behind her, supporting her back: and a third was sitting behind me, and to my left, about two yards off; yet I delivered this woman with my forceps, and neither she nor any of her three attendants knew that she had received any artificial assistance until half an hour after the labour was over. Mother and child did well. Ergot would probably have killed this child, and if I had had to send home for the instrument the delay might equally have been fatal to it.—*Obstetrical Journal, Feb.* 7, 1877, *p.* 725.

82.—USE OF THE FORCEPS IN FIRST STAGE OF LABOUR.

By Dr. J. G. Swayne, Consulting Physician-Accoucheur to the Bristol General Hospital.

"The first stage of labour must be perfectly finished before we think of applying the forceps." This is Denman's fourth aphorism, and is a rule of practice which has held almost undisputed sway for nearly a hundred years. In the present day, however, the motto of the accoucheur may be said to be "Nullius addictus jurare in verba magistri." No truth is considered to be so firmly established that it is taken for granted and allowed to pass without question. The modern accoucheur does not feel bound to swear by a particular rule of practice because it was that of his "old master" at London, Edinburgh, or Dublin, as the case may be, but reserves his judgment until he has had frequent opportunities of testing it for himself by his own experience. This is the case very much with the use of the forceps. In no branch of obstetrics have we departed so much from the precepts and practice of our forefathers as in this. The forceps is now used with much greater freedom than it was formerly, and, as experience has abundantly proved, with the best effects. For instance, about thirty years ago, according to Dr. Churchill's statistics, the forceps was not used in British practice as often as once in three hundred cases. A reluctance to resort to this instrument was at that time the especial characteristic of the Dublin school. This, no doubt, was greatly due to the precept and example of Dr. Joseph Clarke, who was master of the Rotunda Hospital from 1787 to 1793. According to the first report of that hospital published by him, he used the forceps only once in every seven hundred and twenty-eight cases, and his biographer further states that he only used it "once in the multitude of cases under his care in private." Dr. Collins, who was master of the Rotunda from 1826 to 1833, scarcely employed the forceps with greater frequency; for he records but twenty-four forceps cases in a total of 16,414.

In the present day, on the contrary, the Dublin School of Midwifery is pre-eminent for skill and boldness in employing and developing the great capabilities of this most valuable aid to labour. We find, from Dr. George Johnston's report of the Rotunda Hospital for 1869, 1870, and 1871, that, of 3,338 women delivered in the hospital during that period, 227 were assisted by the forceps, being at the rate of 1 in 14·74. This increased use of the forceps is attended, as Dr. Kidd has pointed out, with a diminished maternal mortality, but more especially with a most important saving of infant life, chiefly because the forceps is now employed in Dublin in difficult cases, which would formerly have been delivered by the perforator.

Within the last five years, however, a still more startling innovation has arisen in obstetric practice; viz., the use of the forceps in the first stage of labour. In his report of the Rotunda Hospital for 1872, Dr. George Johnston remarks: "In thirty-five instances, we were obliged to employ the forceps before the os was fully dilated, twenty-seven being primiparæ and eight multiparæ. In thirty of these, the interference was considered necessary, in consequence of the os uteri continuing undilated, apparently the result of the too early rupture of the membranes and the escape of the liquor amnii."

In his report for 1873, Dr. Johnston again gives thirty-six cases in which the forceps was applied before the os uteri was fully dilated, and remarks: "As there may still be many who will be astonished at this apparently bold mode of practice, and mayhap question its justifiability, I beg leave to assure them that, having adopted it for the last two years, during which time we delivered seventy-one such cases, we are more and more convinced every day of its great advantage in saving the lives of both mother and child." He then gives an analysis of the above thirty-six cases, and calculates the amount of expansion of the os uteri in each at the time of the operation, four inches being assumed to be the utmost dilatation of the os uteri, and this diameter of four inches is divided into five parts. "In eleven instances, the forceps was applied when the os was but two-fifths dilated, when, in fact, we were obliged to expand it with our fingers before we could pass the blades, and in every instance both mother and child were saved, with one exception, a case of convulsions, which was brought in comatose. In twenty-two instances, where the os was three-fifths dilated, all the mothers recovered but one, and all the children but two, which were cases of prolapsed funis. In three instances where the os was four-fifths dilated, the mothers recovered and children lived. The position of the head, with regard to the pelvis, at the time when the forceps was employed:—In two cases, the head was above the brim; in fourteen in the brim, and in twenty it was in the cavity. Result: All the mothers recovered but two, one ot which, a primipara, who was very delicate and anæmic on admission, died of peritonitis, with uterine diphtheritis; the other, also a primipara, was admitted comatose and in convulsions."

Before a mode of practice so contrary to all precedent can be regarded with favour by obstetric practitioners, it is necessary that the experience of a great number of observers should be recorded. As a report even of a limited number of cases in private practice is of use in this respect, I propose to give my own experience of it, first premising that I adopted this novel method of using the forceps with my mind strongly prejudiced

against it as a piece of "meddlesome midwifery" of the most dangerous description. The following cases will show whether my prejudice was well founded.

1. About 1 a.m. on July 16th, 1875, I received a message from Mr. James, requesting me to see Mrs. S., Windsor Terrace, Woolcott Park, whom he was attending in her first confinement. The pains first commenced at 9 a.m. on July 14th, and, when I saw her, the os uteri was only dilated to the size of about three inches in diameter. The pains had gone on continuously and she was feeling exhausted. We, therefore, determined to apply the long forceps. The presentation was natural, the head tolerably low in the pelvic cavity, and I could just reach the ear behind the right pubis. I used Simpson's long forceps. There was not much difficulty in applying it, and in less than an hour I delivered her of a male child alive and tolerably vigorous. The os uteri and the perineum presented very little obstacle to the passage of the head. She did well.

2. On July 21st, 1875, I attended Mrs. T., Miles Road, in her third confinement. The labour began at 4 a.m. on July 31st. The first stage was tedious, and, for four hours after the membranes were ruptured, the head remained high up in the pelvis and the os was scarcely dilated to three inches in diameter. As no progress appeared to be made, I applied the long forceps, and, after some difficulty, delivered her of a very large male child, alive and vigorous. The os was dilated to about three inches in diameter when the forceps was applied, but it did not present any difficulty, as it was soft and dilatable. She recovered well.

3. On June 28th, 1876, I attended Mrs. C., Clifton Park Road, in her first confinement. Labour began at 4 a.m. on the day previously, but the os uteri was very rigid, and, though there had been regular pains throughout the day, it was not dilated larger than a shilling at 10 p.m. I was called up to her at 1.30 a.m. The membranes had just ruptured and the pains were much stronger, but the os was not larger than a half-crown. I then felt the anterior fontanelle towards the right acetabulum, and by pressure on the right parietal eminence, succeeded in bringing the occiput round to the left acetabulum. The os uteri still continued very rigid, and by 7 a.m. was not larger than a crown-piece. I therefore used Dr. Barnes's long forceps, and, after some time and trouble, delivered her of a large male infant, alive and well, about 8.45 a.m. The pelvis was not very roomy. Some *post partum* hemorrhage followed. It was restrained by cold, pressure, and ergot. The perineum, notwithstanding careful support, was lacerated nearly to the sphincter. I therefore used three wire sutures. The tear healed by the first intention, and the patient made a good recovery.

4. On September 3rd, 1876, I attended Mrs. S., in Caledonia Place, in her first confinement. She was a blonde, tall, robust in make, and about 36 years of age. Labour commenced five days previously, and the pains of the first stage continued at intervals all that time. The os uteri was rigid and yielded very slowly. About 5 p.m. on September 2nd, it was dilated to the size of a crown. About 9 p.m., it had dilated to the size of the bottom of a tumbler, or a little over two inches in diameter, and the membranes gave way. The pains were regular, but not very frequent. The os uteri continued in much the same state until the next morning, the head being in the pelvic cavity. I could not reach the ear, but I could feel the great fontanelle opposite the right acetabulum. The os uteri was now about two inches and a half in diameter. As the labour had become very tedious, I applied Dr. Barnes's forceps, and, after about four hours, delivered her of a large male child that had apparently been dead for some hours. I concluded that such was the case, because the liquor amnii was much coloured with meconium, and the skin had peeled from a considerable part of the head. The patient made a good recovery.

The above cases, it will be observed, corroborate the testimony which Dr. Johnston has given in favour of the employment of the forceps, under certain circumstances, during the first stage of labour. In all, the result was good as regards the mothers, and also the children, with the exception of No. 4, in which the child was still born; but, in this case, the death of the infant appeared to have occurred before the forceps was used. It will be observed also that, in one case (No. 2, a third confinement), the delay in the labour did not arise from a rigid os, but from a disproportion between the head and the pelvis, causing the head to be arrested at the pelvic brim. The diameter of the os uteri did not exceed three inches, simply because the head did not press down sufficiently upon it after the waters had escaped. This incomplete dilatation of the os uteri in cases of contracted pelvic brim has long been familiar to accoucheurs, and has not been considered to be an obstacle to the performance of craniotomy, or even to the careful employment of the long forceps. It is far different, however, in the other three cases, which were primiparæ, and in which the insufficient dilatation was the result of rigidity. In these, the forceps would have been formerly considered to be quite inadmissible, mainly, no doubt, for two reasons: first, because the dangers and difficulties attending its use are much greater in the first than in the second stage of labour; and, secondly, because, as Dr. Churchill's statistics show, a protracted first stage is not *per se* dangerous either to the mother or the child. He admits,

however, that a protracted first stage may, by inducing fatigue and exhaustion, act unfavourably on the second stage. I had once a well-marked instance of this kind. The patient, a primipara aged 30, had been in labour five days before the os uteri became dilated to the size of a crown. The anterior lip of the rigid os uteri then gave way, and a considerable rent took place. The second stage went on well for a time, until the pains almost ceased from sheer exhaustion. I then applied the forceps and delivered her, but the prostration which ensued was so great that the patient nearly lost her life. In this case, all the usual remedies for relaxing the os uteri had been tried, but without effect. I have little doubt that, if the forceps had been applied during the first stage, the result would have been much better. One great object in using the forceps is to anticipate evil rather than to remove it when it exists. Before adopting craniotomy, the accoucheur should be satisfied that urgent symptoms exist which render prompt delivery imperative. With the forceps, however, it is far different. It is so safe an instrument in moderately skilful hands, that it ought to be employed before any symptoms of powerless labour set in. No one in the present day would think of endangering a woman's life by waiting, as Denman recommended, until the pains of labour had ceased and the head had been six hours as low as the perineum. It is quite a sufficient justification for using the forceps that the progress of the labour has been arrested for a time, and that the head has ceased to advance. When the forceps is thus employed in good time, the accoucheur can take time about the operation and imitate nature as closely as possible; but, if he wait for symptoms of powerless labour, he will have to deliver in too great a hurry, and when his efforts are not efficiently seconded by the pains. In the first stage of labour, above all, the delivery should be effected slowly and cautiously. Urgent symptoms have not yet set in, and the accoucheur can, therefore, afford to wait. He should only extract during the pains, and should not mind if the operation occupy three or even four hours, as it did in my last case. He will thus give ample time for the soft parts to dilate and avoid the danger of lacerations.

My experience, then, of the application of the forceps during the first stage of labour is, that the operation, when properly performed, is safe and very often useful, although seldom imperatively demanded; moreover, that it requires a considerable amount of that *tactus eruditus* which can only be acquired by practice, and, therefore, it should not be performed by any man until he has used the forceps at least a dozen times during the second stage of labour; and I am induced, on the whole, to agree with Dr. Playfair's conclusion that, "if the os be not fully dilated,

but is sufficiently so to admit of the passage of the forceps, the operation, under urgent circumstances, may be quite justifiable, although it must necessarily be a comewhat anxious one."—*British Medical Journal, April* 28, 1877, *p.* 508.

83.—THERAPEUTIC ACTION OF SECALE CORNUTUM.

By Dr. James Cuthill, Assistant to the Extra Physicians at the Royal Hospital for Sick Children, Edinburgh.

The well-known energetic influence of the ergot of rye upon the parturient uterus naturally led to the employment of such a potent drug in various other conditions of the system. It has accordingly been used most notably in the hemorrhages generally in hemoptysis, in hematemesis, in epistaxis, in purpura, &c. More recently, it has been tried in cases of atony of the intestines, accompanied, as such a condition is liable to be, with irregularity in the alvine discharges, and the consequent generation of gases—flatulence in the bowels.

With the view of observing the effect of ergot upon the contractility of the smaller vessels, I have on recent occasions several times put a frog's foot under the microscope, and after, by means of the micrometer, noting the natural size of the vessels, watched the effect of the subcutaneous injection of ergotine, and on each trial have observed that most markedly the arterial radicles were reduced in their calibre to the extent of about one-half, with accompanying stasis of the blood current.

Following out the observation, and remarking that this contractility must necessarily result from the action of the secale upon the fibres of the involuntary or unstriped system of muscles,—an influence which cannot reasonably be confined to one particular region of the body,—and whether we regard such contraction as occurring through the medium of the blood, or by the circuit of the cerebro-spinal or sympathetic systems of nerves, we are equally confined to the deduction that its virtue must be general and not local, and will in like measure be equally developed in whatever quarter of the body organic muscular fibre is plentifully distributed, and that its effects should largely extend to the heart, to the stomach and intestines, to the bladder, to the uterus, to the spleen and other viscera whose anatomical structure to a considerable extent partakes of this substance.

Accordingly, in a heart case which lately presented itself, in which the prominent symptoms were palpitation, with some degree of valvular murmur and increased dulness over the cardiac region—in short, the ordinary signs of simple dilatation of that organ—liquor secalis cornuti was administered in 1 dr.

doses three times a day, with marked benefit to the patient, and the gradual amelioration of all the symptoms. The success of such a case, of course, depended upon the absence of structural changes in the valves themselves, the pathological condition being want of tone in the cardiac muscular apparatus, or flaccid heart as it might be designated, valvular incompetency being caused by dilatation of the orifices occurring synchronously with enlargement of the organ, and not to changes in the size or capability of the valves, which resumed their natural functions when the organ and its orifices returned to their normal dimensions.

I have not as yet had many chances for making clinical observations of a reliable nature on the action of ergot on the other organs indicated, but hope in a future paper to do so, when more extended opportunities present themselves.

In conclusion, I would beg to indicate that, were the effects of different drugs upon the elementary tissues—on muscle, striped and unstriped, on nerve tissue, cerebro-spinal and sympathetic, on areolar and fibrous tissue, and on the osseous and integumentary textures — correctly investigated, much simplicity would be imported into our knowledge of the action of remedies, and a veritable advance be effected on the progress of rational therapeutics.—*Edinburgh Medical Journal, Jan.* 1877, *p.* 600.

84.—AMPUTATION OF HYPERTROPHIED CERVIX UTERI BY ESMARCH'S ELASTIC CONSTRICTOR.

By Dr. A. L. Galabin, Assistant Physician to Guy's Hospital.

Of all the methods which have been usually employed for the amputation of the hypertrophied cervix uteri, the use of the galvanic cautery écraseur has of late obtained a preference which I believe in the main to be thoroughly well deserved. As compared with the old-fashioned chain, or with the wire or wire-rope écraseur, its advantages are manifest. For when the cervix uteri is the subject of chronic engorgement and hypertrophy, or areolar hyperplasia, its tissue becomes, in the later stage of the process, extremely dense and hard. When this tissue is compressed in the loop of cold wire, the resistance, in some cases, becomes enormous, and even wire of considerable strength may be repeatedly broken in the attempt at removal. From the same cause arises the danger of injury to the bladder or peritoneum. Thus it has happened that even distinguished gynæcologists and skilful operators have recorded cases in which they have been so unfortunate as to lay open both the pouch of Douglas and the bladder, or one of these cavities, in amputating the cervix by means of the écraseur. It has been supposed by some that the effect of the great tension is forcibly to draw into

the range of the wire-loop tissues not originally encircled by it, and that the accident has happened in this manner. I can scarcely see, however, how such a result could be mechanically possible, unless the cervix itself, at the point encircled by the wire, had a conical shape, with its broader part downwards, so that the whole wire-loop would tend to slip upwards as it became tightened; but I think there is no doubt that by the extreme constriction great dragging is produced upon surrounding parts, and that the bladder or peritoneal cavity has, in some cases, been actually torn open in this manner, without having been ever included within the circle of the wire. Another drawback to the use of the common écraseur is that it affords by no means an absolute safeguard against even considerable hemorrhage, and that its use has therefore occasionally to be supplemented by the application of the actual cautery, or of plugs with perchloride of iron.

From the risk of endangering the bladder or peritoneal cavity the galvanic cautery is free, provided that the wire of the écraseur has been originally adjusted in a safe position; and, with due regard to a sufficient heating of the wire and a suitable slowness in tightening it, it may be made to afford an absolute security against hemorrhage. The galvanic cautery itself, however, is not without certain disadvantages. First among these may be reckoned the fact that it is not procurable except in large cities; and that its mechanism is apt to get out of order, so that it may be found to fail to work, or to afford a sufficient heating power, just at the moment of greatest need. Moreover, the charred surface of the section must cast off a superficial slough, and a long process of healing must ensue, while the surface is granulating and the mucous membrane slowly extending over it from its margins. The vaginal discharge generally becomes offensive at the time that the slough is separating; and it is probably on this account, from the absorption of some septic material, that a slight periuterine inflammation is occasionally set up, although I have never observed any more serious result than this to happen.

Another unfortunate sequel which occasionally follows from the use of the galvanic cautery is that the granulating surfaces may tend to adhere, and so produce contraction or complete occlusion of the remaining part of the cervical canal. I have recently had an example of this kind in the case of a patient about forty years of age, from whom I removed an epithelioma of the cervix uteri by means of the galvanic cautery écraseur. The uterus at the time was free from any fixation to surrounding parts, but the cervix at the point of section was much broadened; and although it was a question how far this might be the result of a simple hypertrophy due to the stimulus of

the adjacent disease, I had but little hope that I had succeeded in getting above the limits of the malignant growth. About a year after, the patient returned to the hospital, complaining of severe periodic pains, which recurred every month, and lasted from one to two weeks. The vagina was now found to form a perfectly small vault, in which only a minute cicatrix indicated the point where the cervix had been amputated. The uterus, not much distended, was felt above as a small round movable body, but nothing like a cervix could be made out. As the patient was not far from the usual climacteric period, I decided, with the advice of Dr. Braxton Hicks, not to interfere by any operation, unless the distension of the uterus should increase, or the pain become more severe. Six months after the patient again returned. The uterus had now become as large as a small orange; the pains still returned every month, but were longer in duration, and so severe that frequent hypodermic injection of morphia had become necessary. I therefore passed a bistoury into the uterus at the point marked by the cicatrix, evacuated some treacly fluid, and enlarged the opening sufficiently to admit a conical dilator, one fifth of an inch in diameter. The patient had transient symptoms of peritonitis, but was soon convalescent, and menstruation has since recurred normally. There is still no sign of any cancerous growth remaining.

I have not as yet met with any cases in which occlusion of the cervix has followed amputation by the galvanic cautery for simple hypertrophy, but I believe that such have been observed. The occurrence is, no doubt, an exceptional one, but it is so far a danger to be taken into account that Dr. Barnes recommends that an intra-uterine stem-pessary should be introduced for a month after the operation, in order to prevent its taking place. It must be obvious that the introduction of an intra-uterine stem immediately after any operation upon the cervix must tend to increase whatever risk there may be of uterine or peri-uterine irritation, and that any mode of operating in which this need is avoided would so far be preferable. I think, therefore, that it may be worth while to examine the other modes of amputating the cervix, even if they should prove as much open to objection as the galvanic cautery, if only for the sake of practitioners in the country who live beyond the reach of that valuable instrument, or of patients in town who are too poor to afford the extra expense incurred by its use.

To the old methods of amputation by cutting instruments, to that by means of scissors, and still more to that by the knife, the main objection is the difficulty of restraining the hemorrhage, since the usual means of doing so, by means of torsion or ligatures, are rendered impossible on account of the dense

tissue within which the vessels are embedded. Hence it is generally necessary to make use of styptics or the actual cautery, and by this means a slough of the incised surface is produced, and the resulting disadvantages in the sequel are at least as great as after the use of the galvanic cautery écraseur. A far better method, if it can be carried out, is that proposed by Dr. Marion Sims. His plan is to dissect up flaps of mucous membrane from the external part of the cervix, and, after cutting off transversely the redundant portion, to stitch down the flaps upon the incised surface. The object of this method is not merely to avoid a protracted suppuration and cicatrisation by covering the raw surface with flaps, but by their means to apply sufficient pressure to arrest the hemorrhage. Its disadvantage is that it occupies some time to execute it, and that in some cases profuse hemorrhage may be going on all the while until it is completed. A slight modification of the method has been introduced by Prof. Hegar. He cuts his flaps half from the mucous membrane of the exterior of the cervix, half from that of the cervical canal, so that the two meet near the centre of the incised surface. In passing the sutures he brings the thread to the surface for a short space near this point of junction, midway between the two margins, so as to cause an infolding of the dense tissue of the cervix. By this plan he believes that the hemorrhage can be more effectually arrested than by that of Sims. In order to control the bleeding during the course of the operation, which necessarily occupies somewhat longer according to this method, a more complex one than that of Sims, he recommends that the cervix uteri should be encircled and constricted by the serre-noeud made by Cintrat of Paris, an instrument resembling a small écraseur. With the same object Dr. Emmet has introduced an instrument which he terms the uterus tourniquet, whereby, with the aid of a double cannula, the cervix is surrounded by a band of elastic watch-spring, which can be tightened at pleasure.—*Lancet, Feb.* 17, 1877, *p.* 230.

85.—ON THE CONDITION OF THE CERVIX UTERI IN THE LATTER MONTHS OF UTERO-GESTATION.

By Dr. Angus Macdonald, Edinburgh.

Up to the time of Weitbrecht, 1750, it was universally held that the cervix uteri during pregnancy underwent a gradual amount of shortening, which was believed to be so closely correlated to the period of the pregnancy that the shortening of the organ might be taken as a symptom of the period of the pregnancy. It was believed, indeed, that during the early part of pregnancy the body alone was concerned in forming a

receptacle for the fœtus, but that as pregnancy advanced the cervix at its upper part became gradually used up, and was made to contribute successive portions to the amplification of the lower segment of the gravid uterus, until about the natural time for the onset of labour nothing remained of the original cervical cavity except a ring forming the outer os.

The description given by Weitbrecht of a uterus in the eighth month of pregnancy, however, ought to have been sufficient to settle the matter. But, like much other good work, his observations were soon forgotten. Stolz and Caseaux, from observations upon living women near the time of labour, again directed attention to this subject, after the oblivion into which it had unworthily fallen. But it is Duncan to whom the merit of really resuscitating the true views in modern times is due. In his masterly paper on this subject he gathers together the earlier views scattered through works too seldom consulted now-a-days, and furnishes drawings of several dissections from earlier authors, such as Roederer, to which he adds several from dissections made by himself. These drawings from Roederer and John Hunter are especially valuable, as they were correctly made by the authors according to what they found in nature, and are dead against their own defective theoretical ideas.

The views promulgated by Duncan in this country and supported by Müller in Germany may be said to have almost attained to absolute acceptance on the Continent and to very general currency here. The advantage in the position taken by him is that it rests not on theory, but anatomical fact. One fact is surely worth more than a very large amount of theory. And yet it is astonishing how difficult it is for the honestest and best observers to get rid of the slavery of preconceived conceptions, and away from the feeling that their plans are so much better than what is said to be found in nature, that the latter must be interpreted and explained away so as to bring it into agreement with their fancies.

It is accordingly rather disappointing to find an author of such deservedly great reputation as Bandl striving to return to the antiquated notion that the cervix uteri is used up in the latter months of pregnancy to aid in forming the lower uterine segment.

Now it so happens that, about a year ago, one of my dispensary pupils was called to attend a patient in High Street, who believed herself actually in labour at full term. He saw her in the evening he was sent for, and found no pains present, and that she was mistaken in believing her labour had begun. On the forenoon of the following day, on sitting up in bed, she suddenly died. I insisted on a sectio, and with some difficulty procured the consent of her husband to the removal of the

entire uterus with its contained ovum. The post-mortem examination was performed for me by Dr. Wyllie, and the uterus was injected by Professor Turner from both the uterine and fœtal vessels. One half of this uterus is now laid before you. It is to be noted that the patient was forty years old, and that she had given birth thirteen times previously; that she supposed herself arrived at the full term of pregnancy, and, indeed, in labour, so that she must at least have been past the middle of the ninth month, and that the uterine organs were in a condition of absolute health. There was found no apparent cause of death on the sectio, except it had been sudden syncope from a somewhat large, pale, and friable heart. In this case, therefore, no question of the effects of disease can be raised.

At the examination it was noted by Dr. Duncan, who was also present, that the cervix was undeveloped, and the inner os allowed two fingers to pass. I leave the members of the Society to judge whether there is any evidence here either of the obliteration of the original cervix, or of the formation of a new cervix, such as Bandl would make us believe, or of a squashing down of the cervix by an opening out of the upper part of it, as Farre and M'Clintock would seem to believe in. Bandl's line of demarcation between his new cervix and the body of the uterus is conspicuous only by its absence. It will be noted, that the mucous membrane of the cervical cavity is still exactly as it had been throughout the pregnancy. The *arbor vitæ* arrangement is still seen, and the line of demarcation between the decidua and the cervical mucous membrane is as perfect as it could have been at any time during the pregnancy. The membranes have been raised by Mr. Turner from the mucous membrane for a little space upwards from the line of junction between the cervical mucous membrane and that of the body merely to show the decidua, but they were originally found intact and attached down to the very edge of the inner os. The length of the cavity of the cervix is, at its longest part, where the lips project most, $1\frac{1}{2}$ inches; at the junction of the one lip with the other, it measures only $1\frac{1}{4}$ inches. The thickness of the cervical tissues amounts to $\frac{7}{10}$ths of an inch, which is decidedly greater than that of the body of the uterus, which measures only a quarter of an inch in thickness. Of course the dimensions are no doubt lessened in all directions by the preparation being kept in spirit for a considerable time.

I have further to observe, that the vaginal portion of the cervix is still markedly persistent, and that the thickness of the cervical wall, with the unused cervical portion, lend a considerable probability to the conviction, if that were needed, that Bandl's fears about want of tissue out of which to form the cervical segment of the fully dilated genital passage are groundless.

I have thus laid before the Society this very interesting and rare specimen, and do not mean to enlarge further on the subject than to state, that it seems to me to put beyond a doubt that, under ordinary circumstances, the cervix uteri neither shortens nor has its cavity obliterated during pregnancy. On the other hand, it manifestly shows that the cervix, while partaking of the general hypertrophy of the genital organs during pregnancy, maintains its general shape intact till very near the termination of pregnancy.—*Edinburgh Medical Journal, April* 1877, *p.* 870.

86.—EXTRA-UTERINE FŒTATION; ABDOMINAL SECTION; RECOVERY OF MOTHER AND CHILD.

By Thomas R. Jessop, Esq., Surgeon to the Leeds General Infirmary.

[Mary C., a married woman, believed herself to be two months pregnant of her second child, in the early part of March, 1875. At this time she experienced a wrench in the right side of her body, in lifting a heavy washing machine. This was followed by much abdominal pain.]

On the 10th of March, shortly after commencing to wash, she had occasion to go to the water-closet, and whilst there, but not in consequence of any effort or straining, she was suddenly seized with a violent pain in the right side of the belly, which caused her to swoon away. She was immediately carried to bed, and Mr. Clayton, her ordinary medical attendant, was sent for. As the symptoms presented were those of inflammation—pain, swelling of the abdomen, vomiting, constipation, retention of urine, and fever — she was treated accordingly. For two months she was confined for the most part to bed, suffering constantly from abdominal pain, sickness, and loss of appetite.

On the 14th of May she became conscious for the first time of the movements of a child, and at about the same time she observed a hard swelling in the lower part of the body towards the right side. On the 18th she obtained admission into the Cookridge Convalescent Hospital, near Leeds; but she returned home after eight days, because her sufferings compelled her to lie in bed longer than was permitted by the rules of the institution. From this time forward she continued to suffer from constant abdominal pain, frequent sickness, constipation, and want of appetite. Her pains were throughout aggravated by every movement of the child, and she was in perpetual dread of any such movement.

On three or four different occasions a discharge of blood from

the vagina took place, extending once over a day and a night. Her flesh and strength rapidly wasted. From time to time the attacks of pain and vomiting became more violent, and when, at length, it was evident that her life was becoming endangered, Mr. Clayton, who for some time past had formed the opinion that the child was lying outside the uterus, sought the advice of Mr. Samuel Hey. These gentlemen made a careful examination of the case together on the 13th of August. They satisfied themselves of the existence of an extra-uterine living fœtus, and gave it as their opinion that an operation for its removal was called for. Seeing that it was my week for admitting patients at the infirmary, Mr. Hey called upon me, and asked me to undertake the treatment. She was admitted into my ward on the 13th of August, 1875.

I found her looking emaciated and pain-worn, vomiting after everything swallowed, and constipated, feverish with a dry tongue, great thirst, and a rapid feeble pulse. It was abundantly evident that the patient was fast sinking.

At eleven p.m. a consultation of the whole surgical staff was held, at which the following observations were made. The abdomen throughout was distended. At the umbilicus and below was a large rounded prominence, which gradually sloped off towards the ensiform cartilage, and terminated inferiorly somewhat abruptly in a hollow, which was bounded again by a lesser prominence immediately above the pubes. On a closer examination, the umbilical prominence presented the character of a child's breech. The cleft and the two buttocks were distinctly traceable through the thin abdominal walls, and extending upwards in a straight line towards the sternum, the little prominences of the vertebral spinous processes were plainly perceptible. Above the pubes two feet could be made out, and above the umbilicus, immediately below the ribs, it was not difficult to map out the outines of the two scapulæ. The rapid beating of the fœtal heart could be most distinctly heard towards the right side above the umbilicus. The breasts were enlarged, and the areolæ were fairly developed.

On examination per vaginum the uterus felt somewhat enlarged, and on measurement by Simpson's sound its cavity was found to be two inches and a quarter in length. The uterus remained motionless whilst the abdominal contents were swayed from side to side. On several occasions the movements of the child were plainly visible, and indicated considerable vigour. After repeated careful search we were unable to satisfy ourselves of the presence of a placental souffle. The diagnosis of extra-uterine gestation seemed complete. The woman's condition was becoming extremely critical. Under these circumstances it was decided to remove the child by abdominal section.

With the full concurrence of my colleagues, I accordingly proceeded to perform the operation at half-past 12 on the morning of the 14th August.

The patient having been placed under the influence of ether, and the bladder emptied of urine, an incision six inches long was made through the linea alba, with the umbilicus at its centre. The abdominal wall was unusually thin, but more vascular than common, and the peritoneal lining, though natural on its free surface, appeared thick and velvety on section. Immediately upon the completion of the incision, the breech and back of the child, thickly coated with vernix caseosa, came directly into view. At the upper part of the wound the omentum was seen lying like a cap upon the child's shoulders; and inferiorly the funis, of natural appearance, passed transversely across the wound, and was traced round the external aspect of the left thigh of the fœtus to its attachment at the umbilicus.

The child was in a kneeling position, its breech presenting towards the mother's navel, its head folded upon its chest, buried beneath the omentum and transverse colon, the soles of its feet pointing towards the pubes, and its knees resting upon the posterior brim of the pelvis. Its removal was readily effected. The funis was tied and separated in the usual manner, and the child was handed over to the custody of two gentlemen previously appointed to look after its well-being. It was now seen that the gestation had been of the "abdominal" variety; no trace of cyst or of membrane could be found. The child had lodged in the midst of the bowels, free in the cavity of the abdomen.

A few bands of unorganised lymph of a very friable nature, lying upon, but not adherent to, intestines, were readily removed by sponging, and about an ounce of clear serum was found in the peritoneal cavity. On tracing the umbilical cord, the placenta, having a larger superficial area than natural, was seen covering the inlet of the pelvis, like the lid of a pot, and extending some distance posteriorly above the brim, where it apparently had an attachment to the large bowel and posterior abdominal wall. Near its centre was a round prominence, which seemed to correspond with the swollen fundus of the uterus beneath. Great and especial care was taken not to cause the smallest disturbance to its connexions. The placenta was indeed left *untouched*[1] The umbilical cord was now brought out of the wound and shortened, so as to have its cut end protruding about two inches beyond the surface of the abdomen, where it was secured at the inferior extremity of the wound by means of a clamp which has been invented by the ingenious chaplain to our infirmary—Mr. Gough—for the treatment of

the pedicle in ovariotomy. The wound was now closed by means of six silver-wire sutures passed through the entire thickness of the abdominal wall, and including the peritoneum, together with as many intermediate superficial sutures of silk. It was then observed that the prominence above the pubes before alluded to was due to the placenta covering the enlarged uterus. Strips of plaster, pads of lint, and a roller completed the dressings.

The child, a female, was well developed, considering that in all probability it had not reached the eighth month of fœtal life. For a considerable period of time—from an hour to an hour and a half—there was much uncertainty as to its survival owing to very defective respiration. Perhaps this might have been due, as was suggested at the time by Mr. Hey, to its having become etherised or in other manner narcotised through the medium of the mother's blood. At length, through the skilful management of Mr. Hey and Mr. Scattergood, healthy breathing became established, and the immediate danger to the child's life was averted. In the course of a day or two a wet nurse was procured for it, and afterwards its progress became most satisfactory.

I will not run the risk of wearying you by a recital from my note book of the mother's condition from day to day. The further history of the case shall be dealt with briefly.

For the first four days it was deemed prudent to keep the patient upon the table in the operating-room, lest her removal should prove prejudicial. Afterwards she was placed in a private ward. Morphia was injected subcutaneously, at first freely and frequently, subsequently at longer intervals, and on the 26th of September, six weeks after the operation, it was finally abandoned. During the first two days nothing was given by the mouth but a little ice. On the day after the operation, nutrient injections were commenced, and they were continued for about four weeks, being given at first once in four hours, and subsequently at increasingly long intervals. On the morning of August 16th a little flatus, and on the 18th some fæces, passed from the bowels. From the time of the operation the vomiting gradually lessened in frequency and in severity, and by August 17th it had ceased to follow upon each act of swallowing, and had become an occasional symptom only.

On the 17th, too, the clamp was removed, and the funis, in a gangrenous state, could be traced deeply through the wound. To prevent it from dropping into the abdomen it was secured outside by means of jute and adhesive plaster. From time to time she complained, when not under the influence of morphia, of very great pain at the bottom of the body and in the thighs.

There was not at any time a discharge from the vagina, although she made complaint of pain and forcing as if due to uterine contraction. On the 18th of August her breast became distended with milk, and by the 22nd it had disappeared again. Besides the nutrient enemas she now began to take a few spoonfuls of milk and gruel by the mouth. On 19th August, five days after the operation, the dressings were found soaked with a bloody discharge. This continued daily for some time in quantities varying from one or two, to as much as eight or ten ounces. On the 22nd it was observed to be offensive, and of a thick, dark, grumous character; and in a few days later the stench arising from it was most intense. On 24th August she had a severe rigor, lasting ten minutes, and this was followed by vomiting.

On August 29th the vomiting had ceased, and her general condition had so far improved that her request to have tea and a biscuit was complied with. From this time her diet was improved daily—fish, eggs, chickens, &c., being cautiously added at intervals.

On September 4th the funis was cast off as a long slender slough, its discharge being followed by a copious flow (six or eight ounces) of fluid.

The wound had now healed in its entire length, with the exception of the round hole at its lower extremity, which had previously been occupied by the umbilical cord. At this time three weeks had elapsed since the operation. During the next fortnight the discharge was most profuse. Two, three, and even four times in the twenty-four hours she was seized with agonizing abdominal pains, which, after lasting from a few minutes to two or three hours, were at once relieved by an outpouring of a quantity of putrescent fluid. On the 10th of September I stood by her in one of her most severe attacks, and I was almost alarmed, upon hearing her exclaim, "There, I shall get relief," to see not less than half a pint of coffee-coloured fluid, of the consistency of treacle and unbearably offensive, rapidly forced out of the wound.

On September 14th she complained of pain in the right leg and foot, and upon examination these were found to be slightly œdematous. There was also some tenderness along the course of the femoral vein in Scarpa's triangle. On the 15th a slough three inches long came away. On the 16th the discharge became purulent and decidedly less in quantity. It was observed that the supra-pubic prominence had now disappeared. By the 18th the œdema in the leg had gone, and on the 19th the patient sat up in bed to dinner. Her progress now became steady and equable. On the 27th of September she sat for an hour in a chair, and on the 9th of October she was able to be

transferred to the general ward. The discharge had now become small in quantity, thin, and serous in character. On the 29th of October the wound is reported as quite healed, and three weeks later she returned to her home. From that time to the present, she has kept in good health. Menstruation commenced about a month after she left the infirmary, and has recurred at regular periods ever since.

The child was as healthy, vigorous, and large as an average child born in the natural way, and it continued to thrive well till July, 1876, when, after a week's illness, it died of croup and inflammation of the lungs, at the age of eleven months.—*Obstetrical Journal, December* 1876, *p.* 610.

87.—ON SOME OF THE RELATIONS OF THE FŒTAL HEAD TO RUPTURE OF THE PERINEUM AND INJURIES OF THE EXTERNAL GENITAL ORGANS.

By Dr. J. Matthews Duncan, Edinburgh.

The fœtal head does not come through the vaginal and vulvar orifices simply or exactly in the line of direction of its long axis. That is prevented by the connexion with it of the neck, which forbids the amount of flexion-like movement that might occur if the neck and body were imagined to be absent. Besides, in consequence of the presence of the neck of the fœtus, and from the construction of the passage, the head follows the line of the so-called circle of Carus; not a straight course even at its moment of emergence from the genital canal. With its suboccipital region pressed against the middle of the pelvic arch, and advancing slowly, wheeling around a point somewhere about the centre of the symphysis pubis, the region of its vertex is advancing rapidly in a larger circle, wheeling around the same point, and distending the perineum to its utmost.

Every practitioner supporting the perineum during the birth of the head must frequently have observed that it has retained its entirety until the projecting forehead came on, and this lacerated it. Now, this is easily explained by the table of measurements, for it is not till the suboccipito-frontal diameter is passing that the perineum is stretched to the utmost. The suboccipito-vertical and suboccipito-bregmatic diameters may both pass over the perineum without tearing it, and then the large suboccipito-frontal may lacerate it on account of its greater dimensions. No doubt the suboccipito-bregmatic or the suboccipito-vertical diameter may tear the perineum, or even an earlier passing part; and in this case the tear may be extended and extended, as each larger part follows, till the largest suboccipito-frontal emerges, completing the injury done.

The order in which all the lacerations of the external genital

organs take place I cannot decide, but the following statements are either self-evident or nearly certain.

Before the suboccipito-vertical diameter has passed the vaginal opening, or even when only a caput succedaneum protrudes, the vaginal orifice is lacerated posteriorly. The laceration increases as the head advances.

While the parietal protuberances pass through the vaginal and vulvar orifices, they may lacerate one or both sides of the vaginal orifice or of the vulvar orifice.

The vestibular lacerations take place before the suboccipito-frontal diameter has passed the vulvar orifice. Their occurrence helps to save the perineum from laceration.

A laceration of the fourchette may take place early in the passage of the head.

An extensive laceration of the perineum (say three-fourths of an inch or more) obviates tension of the vaginal and vulvar orifices, and therefore prevents further laceration after its occurrence.

The perineum is the part last lacerated.

Cases of ordinary or characteristic central perineal rupture, in which the head advances in its natural way, and is born through the vaginal and vulvar orifices, and, still better, cases of incomplete central rupture, such as I have recorded in a former paper, require special consideration. They show nearly with certainty that the anterior part of the perineum is in some cases, including themselves, considerably more extensible than its middle or posterior part; for in them the anterior part or a portion of it (it may not be the most anterior part or fourchette) remains entire, while the suboccipito-frontal diameter passes over it, and after the middle of the perineum has given way under the distension by the same diameter of the fœtal head.

In the cases of complete central rupture which I have seen there were two perineal ruptures; first, of course, the central rupture; and, second, an ordinary rupture of the fourchette or anterior part of the perineum. Between them, in the most characteristic cases, but not in every instance, there is an entire strap or bridge of skin unhurt. No doubt the unhurt bridge or isthmus of skin may, in some cases be absent, and the two distinct ruptures be confused in one. Whether this be the case or not, it is to be remarked that both tears may go on simultaneously, and so rapidly that the observer may be unable to distinguish their separate occurrence. While the suboccipito-vertical or even a smaller circumference is distending, and, it may be, tearing the vaginal and vulvar orifices, the hitherto entire middle of the perineum may be at the same time suffering central rupture by the great advancing suboccipito-frontal

circumference. The middle of the perineum may have endured dilatation by the suboccipito-vertical and suboccipito-bregmatic circumferences which have advanced to lacerate the vaginal and vulvar orifices, but cannot endure further distension by the suboccipito-frontal, which therefore tears it, and may yet pass over some of the anterior part of the perineum without destroying such anterior part.

Having discussed the injuries of the perineum produced by the passing fœtal head, we have now to devote attention to the injuries of the fœtal head, caused by the resisting perineum; for it may be held as quite certain that every obstacle or modification of resistance to the advancing head leaves its mark upon it. In the case of difficult passage through the pelvic brim, the changes or injuries are often well marked, and have been carefully studied, with great advantage to the progress of obstetrical science. But the vaginal and vulvar orifices and the perineum also do in like manner produce their peculiar capita succedanea, equitations of bones and shears; and they demand more careful study than has yet been accorded to them. In the meantime I can only enter upon some general views.

The caput succedaneum of the vulvar orifice, or of the end of the second stage of labour, I have already elsewhere described. That the perineum and vaginal and vulvar orifices may by their undilatability lead to equitation of bones there is no doubt, but I have not studied the subject so as to enable me to describe any peculiarity in such overlapping. In regard to the shear produced by the forcing of the head over a perineum, and through a vaginal and vulvar orifice difficult of dilatation, it may be stated that the lateral or antero-posterior part of the ordinary shear is in great measure due to the resistance of these parts.

The mortality of male children in birth, and shortly after it, is greater than that of females. Males more frequently demand artificial assistance in delivery than females. There is no doubt that both these evil conditions are in part owing to the greater size of the male head, and the consequent need of more dilatability and more actual distension of the perineum and vaginal and vulvar orifices than is required by females, circumstances which imply greater injury of the fœtal heads of males.

It has been shown that the part of the fœtal head which most distends the perineum and vaginal and vulvar orifices is that generally called the suboccipito-bregmatic diameter or circumference (including the suboccipito-vertical, suboccipito-bregmatic, and suboccipito-frontal diameters or circumferences of this paper). Hence it should be expected that the suboccipito-bregmatic diameter should show evidence of special compression, and this has been admirably done by Budin. His measurements

were made to show the amount of resilition gradually effected within about two days after delivery, giving thus a satisfactory indication of the amount of compression during birth. "The augmentation (says he) of the suboccipito-bregmatic diameter has been constant (in all his fifty-two cases measured); this augmentation has been almost always very considerable, and has amounted even to twelve mms. The suboccipito-bregmatic diameter is, in fact, that which increases the most after delivery."

In accordance with what has been stated in this paper regarding the suboccipito-frontal diameter or circumference I have no special measurements to adduce in evidence, but I dare say every one will be prepared to believe in its particular diminution during labour, for the lowness of the forehead in a much-compressed head is well known to accoucheurs, and is often a horrid sight to the parents ignorant of its temporary character. But in regard to the suboccipito-vertical diameter or circumference, I have valuable measurements by Budin to adduce, whose evidence quite supports the line of demonstration pursued in this paper. As already stated, the parietal protuberances are embraced in the suboccipito-vertical circumference, and Budin's measurements indicate the amount of diminution of the biparietal diameter, which is one measure of the suboccipito-vertical circumference. Budin describes, and his measurements show the bitemporal diameter, which corresponds to the suboccipito-bregmatic proper, more diminished during labour than the biparietal. Or, to use his own words, "The biparietal diameter also augments after delivery, but this augmentation is not constant; it is by much the least considerable: in fifty-two cases, two times only has it amounted to five mms., once to six, and once to seven mms."

If the argument of this paper is correct, then there should be a great difference between the fœtal heads of primiparæ and those of multiparæ. The greater injuries of the perineum and external genital organs in primiparæ than in multiparæ should be correspondingly reflected in the heads of the fœtuses born in primiparæ and in multiparæ. And so it is. The mortality in birth, and shortly after it, of children born of primiparæ, is greater than of children born of multiparæ. The heads of the children of primiparæ are more deformed by labour than those of multiparæ. Budin, our best authority on this point, writes as follows:—"That this resistance of the soft parts—and Professor Depaul, following M. Dubois, insists strongly on this point—that this resistance of the soft parts causes, during labour in primiparæ, a longer duration of the period of expulsion, is a fact about which there can be no doubt. From it there results also a more marked deformation of the head.

The figures which we have given, and which show considerable modifications in the form of the cranium, are all the consequence of deliveries in primiparæ. In multiparæ, on the contrary, the deformations are much less remarkable, and often are almost absent."—*Edinburgh Med. Jour., Feb.* 1877, *p.* 674.

88.—ON THE LOCAL TREATMENT OF PUERPERAL FEVER.

By the Editor of the Medical Times and Gazette.

Starting from the stand-point that the febrile affections which follow parturition, and which are generally comprehended in the expression "puerperal fever," are due to the introduction of septic matter from without, and only acquire their specific character from their seat of origin, and from the peculiar condition of the generative organs existing at the time, Dr. Heinrich Fritsch, of Halle, recommends the systematic adoption of certain antiseptic measures, not only with a view to prevent infection by the hand of the accoucheur or midwife, but also to remove and destroy any decomposing secretions which may develope in the uterus itself. In the first place, before any examination is made, the hands are to be well washed with soap and then scrubbed with a nailbrush and a solution of carbolic acid. The most convenient arrangement for preparing this solution is to have a number of thirty-gramme bottles filled with concentrated carbolic acid dissolved in glycerine (thirty grammes acid to three glycerine), and to take one or two in the pocket when called to a midwifery case. By measuring out a litre of water into a basin, and adding the contents of one bottle, a solution of a proper strength is at once ready for use. Any instruments that have to be applied must also be previously disinfected. Instead of ordinary oil, carbolic oil must alone be employed to grease the hands and instruments. The patient herself is, if possible, prepared by a sitz-bath, in which the vulva and perineum are carefully cleansed with soap. The vagina is then washed out with carbolic acid solution, with the remainder of which the vulva is washed for the second time, and any incrusted hairs are, if necessary, removed with the scissors. Contrary to what might be theoretically expected, the injection of dilute carbolic acid into the vagina does not render the parts either rigid or rough, and the finger can scarcely distinguish between their condition before and after the irrigation. After the birth of the child, attention must especially be directed to maintaining a free escape for the lochia. In the ordinary position of the patient, with the buttocks slightly depressed below the level of the surrounding portion of the mattress, the lowest part of the hollow of the sacrum is lower than the posterior commissure, so that liquid

has to collect and rise to a certain level before it can escape outwards. Hence, a greater or smaller quantity will always remain behind if nature be left to herself, and may thus set up decomposition in the secretions which escape from the uterus later on. Moreover, the lochia tend to escape at the upper part of the vulvar aperture, and not at the posterior commissure, owing to the way in which the labia overlap and become adherent to one another, so that a considerable quantity of fluid may be retained in the vagina, and there undergo those changes which are so liable to give rise to septic infection. On these and other considerations Dr. Fritsch founds his method of vaginal and uterine irrigation, which consists in injecting a 2 per cent. solution of carbolic acid (salicylic acid has been found to be too feeble an antiseptic), at a temperature of 88° Fahr., into the generative cavity by means of an irrigator containing at least a litre (two pints) of liquid. The instrument must not be raised higher than one and a half to two feet above the patient's body, and it may be necessary to use two or three litres of the solution before the liquid returns completely colourless. For the injection of the uterus, Fritsch finds the best form of catheter is one made of German silver, thirty centimetres long and six centimetres in circumference (to be obtained from Baumgartel, of Halle), and which has a curve somewhat greater than the midwifery forceps. In those cases in which there is slight difficulty in introducing the catheter, there is the greatest probability of the retention of fluid in the uterus, owing to kicking from anteversion of the organ. The introduction of the catheter must be invariably controlled by the finger in the vagina. The ordinary dangers which are supposed to result from post-partum digital examination are prevented by the presence of the antiseptic, and the finger is able to detect and to loosen adherent clots, which irrigation alone has not force enough to detach, and which might become sources of putrefaction and infection if allowed to remain. Dr. Fritsch ordinarily irrigates three times a day, at six, one, and eight o'clock; while the vagina is washed out by the nurse every three hours. While vaginal irrigation should be a *sine quâ non* in the after-treatment of every parturient woman, Dr. Fritsch limits the irrigation of the uterus to cases where an operation has been performed, especially where the fœtus was dead and decomposed; to cases in which liquor ferri perchloridi has been injected into the uterus to arrest hemorrhage, and in which the formation of clots is an essential concomitant of the treatment; and lastly, to cases in which fever has existed for several days when the practitioner is called in (as may be the case when the woman has been delivered by a midwife), provided the lochia are still present.

The advantages derived from irrigation under such circumstances may be summed up as follows:—1. After one or two injections the vulvar aperture ceases to be painful and tender, even though it may have undergone considerable laceration; the introduction of the finger becomes painless, œdema rapidly subsides, and wounds heal without the formation of a definite granulating surface. 2. The lochial discharge rapidly diminishes in quantity, the only exception being when irrigation is used after the injection of perchloride of iron into the uterus, their prolongation being here probably dependent upon the destructive action of the perchloride on the tissues of the uterus, so that a considerable period must elapse before the whole of the sloughs are thrown off. 3. The uterus undergoes involution quicker than under expectant treatment. 4. *The temperature falls after the first injection.* The influence of irrigation on the pulse is less marked, especially if there has already been severe hemorrhage or fever of several days' duration. By this method Dr Fritsch has succeeded in saving severe cases of puerperal fever in which the first injection was made on the fourth to the sixth day after delivery, with a temperature of 40° Cent. (104° Fahr.) or higher; and his experience as assistant for many years in the obstetric clinic at Halle is sufficient guarantee for his statement that these cases were really of the most serious kind. The irrigation of the uterus must be continued for at least five days, or in any case until the complete cessation of febrile symptoms. As a rule, Dr. Fritsch irrigates the uterus for the last time on the second evening on which the temperature remains normal, but the vaginal injections are continued for another week, as occasionally fetid lochia and a return of fever have been observed when they have been left off too soon. There need be no fear that pain or secondary hemorhage will be excited by irrigation; practically, they are not found to occur. The patients themselves make no objection to the treatment, provided the medical attendant explains to them its object. Collapse only occurs where there has been much loss of blood or several days of fever, and in such mainly when the liquid has been injected too cold. In carrying out the method much will depend on the tact and care of the medical man. A few spoonfuls of wine may be given before and after the injection. Internally it is well to give quinine. Salicylate of soda in such small doses as two grammes administered in the course of four hours caused so much delirium and collapse in women after childbirth as to deter Dr. Fritsch from repeating its use.

We have entered at some length into the details of the above method because it seems to us to mark a distinct advance in obstetric surgery, and to hold out a real hope of success by taking advantage of a rational indication for the prevention,

and even the cure, of puerperal fever, where the use of other remedies has as yet been scarcely more than empirical and unscientific. If the great "peril of childbirth" can be lessened by means so simple, the introduction of antiseptic midwifery will be a boon indeed. The need is pressing, for every year many a young wife and mother is snatched away, the victim of puerperal infection.—*Medical Times and Gazette, April* 7, 1877, *p.* 367.

89.—ON THE AVOIDANCE OF PUERPERAL FEVER.

By Dr. W. O. Priestley, President of the Obstetrical Society of London, in 1876.

[The following is taken from the Presidential address:—]

To secure the isolation of a lying-in woman from noxious influences which may be communicated to her from without may be difficult, and in some cases perhaps impossible; but as Mr. Callender has informed us that surgeons have learned to banish, for all practicable purposes, those affections which may be termed septicæmic from surgical wards of hospitals, so I cannot help thinking that the accoucheur will in time succeed in preventing the like affections in puerperal women, so far, at least, as the heterogenetic cases are concerned. We are all agreed as to the absolute necessity of preventing, directly or indirectly, any communication between cases of erysipelas and puerperal patients. We are agreed also as to the necessity of the midwifery practitioner avoiding all post-mortem examinations. His hands should avoid all contact with specific poisons or septic materials; and if perchance his fingers have touched anything suspicious, he should at once carefully and thoroughly disinfect them. The necropsies which seem most baneful are of those bodies which have recently died of erysipelas, peritonitis, zymotic disease, or any other inflammatory and febrile affections; and in such instances the accoucheur should not even be present in the room when the dissection is made, as, though he decline to touch, yet his person and clothes may become infected by the poison. There may possibly be a doubt whether one who dissects bodies altered by decomposition or antiseptic injections, as they are commonly found in the dissecting room of medical schools, is so liable to contaminate a puerperal patient as he who makes a necropsy soon after death. Semmelweiss, however, believed that animal matter in a state of putrefaction would cause puerperal fever, and immediately lessened the mortality, during the fearful epidemic of puerperal fever which raged in the Vienna Hospital in 1846 and 1847, by obliging all dissecting students to wash their hands in chlorine

or chloride of lime before making vaginal examinations. Impressed by Semmelweiss's views and opinions, I endeavoured, during my occupancy of the chair of Obstetric Medicine at King's College, to prevent students who were frequenting the dissecting-room and post-mortem theatre from attending midwifery cases, and the result was a very small mortality in the outdoor maternity charity.

This care about absolute cleanliness of the hands in attending labours should extend even to occasions when a medical man has had digitally to examine patients suffering from offensive discharges either proceeding from the surface or from the interior of the body. We have heard in this discussion that the discharges from cancer may produce fever in a lying-in woman; and this is probably true of other putrescent discharges. I have had grave anxiety, extending over some weeks, about a patient after her delivery; because I had gone straight from an instrumental case where labour had been so protracted that febrile symptoms had set in, and the passages had become inflamed, so as to pour out an irritating, muco-purulent discharge.

When a medical man has a bad case of puerperal fever in his own practice, or is required to see one in consultation, he should certainly not go direct to another midwifery patient without first changing his clothes, besides careful ablution of his hands; and he will be all the safer in his ministrations if he adopt some of those precautions mentioned by Dr. Swayne and others, such as using a Turkish bath, or the more radical measures for disinfection recommended by Dr. Wynn Williams, and for the details of which I may refer to his speech.

Many chemical substances have been recommended to be added to water for purifying the hands: iodine, chlorine and its compounds, sulphurous acid, cyanide of potassium, carbolic acid, and the permanganates. It matters not which agent is employed, so long as it is used carefully and efficiently; and if it be necessary to disinfect clothing, this is readily done by exposing it in an oven to a high temperature, for which many upholsterers have a suitable arrangement. With all deference to Dr. Matthews Duncan, I have no doubt that every medical practitioner who earnestly desires to promote the welfare of his patients and to keep himself blameless should at once abstain from attending fresh cases of labour when one or more of those recently delivered by him have died with acute febrile symptoms, or are still so ill as to require his constant visits. When, indeed, there is the remotest suspicion that he may convey the poison in his own person he should not incur the risk of disseminating it. How long he ought to absent himself from midwifery practice is yet a moot point. Dr. Swayne thinks a medical man should seclude himself for a week only; others

think a month hardly sufficient. The remarkable experience of Dr. Huntley, of Yarrow, led him to believe that he actually generated poison in his own body, as the result of some faulty process, and then communicated it to his patients. Taking all the circumstances into consideration, I am disposed to recommend a week's seclusion after regular attendance has ceased on a single puerperal fever case. When a series of cases have occurred in the practice of any one medical man, he should absent himself from midwifery practice for a month at least. During either the short or the longer interval of seclusion, the means for disinfection should be fully carried out.—*Obstetrical Transactions*, *vol.* xviii., 1876, *p.* 53.

90.—REMOVAL OF LARGE FIBROID UTERUS WITH OUTGROWTHS AND BOTH OVARIES: RECOVERY.

By J. Knowsley Thornton, Esq., Surgeon to the Samaritan Free Hospital for Women and Children.

Mrs. H., aged thirty-eight, came to me in September, 1875, stating that she had an abdominal tumour. She was a native of London, and had been married seventeen years to a Frenchman, by trade a tailor. During the last twelve years they had been residing in Canada. She had never been pregnant, and menstruation, which commenced at fifteen, had been regular, scanty, lasting two or three days, and accompanied by a good deal of pain in abdomen, back, and under left breast. About four years before I saw her she began to suffer from more constant pain in the back, and from a general sense of weakness and inability to go about as usual. Eighteen months later she noticed increase of size, and found a hard lump low down in the left side of her abdomen. This lump grew steadily but slowly larger, while the pain both at and between the monthly periods became so severe that she was unable to attend to her domestic duties, or to walk about without assistance. Her appetite became bad, bowels confined, and her nights restless from pain, which various sedatives failed to relieve. She consulted many doctors in Canada, and they all told her she was suffering from tumour of the womb, and any operation for its removal would kill her. She accordingly determined to come back to her native city, and see if she could get relief.

During the last few years before her return to England, and while consulting various medical men, she had three times had small intra-uterine polypi removed, but she could not remember the dates.

When she first came to me she was thin and worn, face deeply furrowed, and she walked with difficulty even when supported, the body bent forward and sideways. On examining

the abdomen I found a hard movable mass in the left iliac region, extending slightly across the linea alba; pressing and moving it caused her considerable pain. In the right side was another similar, but much smaller and more tender, lump, and the two appeared to have some deep pelvic connexion. By vaginal examination I found a small hard and movable cervix, the os pointing rather to the right side, and having a small pediculated polypus hanging from it. Tracing up from the cervix, I felt some small nodular projections just at its junction with the body of the uterus, and then what appeared to be the body of the uterus much enlarged and displaced to the left side, this being, in fact, the left side abdominal tumour. The right side and back part of the pelvis I found occupied by a hard, elastic, globular mass, which appeared to arise from the enlarged uterus on its right side just above the vaginal portion of the cervix. It seemed nearly to fill the pelvis, and was but slightly movable with much pain to the patient; the lump in the right side was evidently the abdominal part of this mass. The sound passed nearly five inches into the mass on the left side. I have said that the mass in the right side was but slightly mobile; it was sufficiently so, however, to make me feel pretty sure it was only wedged, not adherent.

My diagnosis was, much enlarged uterus with outgrowths, the largest occupying the pelvic cavity.

I at once told the patient that I quite agreed with her Canadian advisers, and I at the same time recommended palliative treatment, to which she somewhat unwillingly consented. During the following three months I saw her occasionally, and each time found her worse than when last seen. At one time she had a smart attack of pelvic peritonitis, due, I believe, to an attempt being made, in the out-patient room of another hospital, to push up the pelvic portion of the tumour. I was puzzled to account for the attack at the time, and only learnt this afterwards, as she had not liked to tell me she had gone to another hospital while I was treating her.

I now became convinced that the pelvic mass was increasing in size, and would soon become a source of absolute danger to her, and her health seemed breaking so fast from constant pain, though she had no hemorrhage, that I consented, at the earnest entreaty of her husband and herself, to take her into the hospital. Mr. Spencer Wells, Dr. Savage, and Dr. Bantock all at different times saw her with me, and all agreed with my diagnosis, and that removal of the tumour was the only possible cure, though not by any means a hopeful proceeding.

Having fully explained the danger to both the patient and her husband—though it was hardly necessary, so well did she understand her own case,—I consented to make an exploratory

incision, and see if I could remove the tumour, promising that if I found it impossible I would endeavour to pull up the pelvic portion, and fasten it to the abdominal wall, to avoid the risk from its impaction in the pelvis.

The operation was performed at the Samaritan Hospital at 2.30 p.m. on January 10, 1877, some of my colleagues and many other gentlemen being present.

Feeling sure that I should have to cut across the cervical canal, I took the precaution to thoroughly cleanse both it and the vagina just before the operation—the former with cotton-wool and carbolised oil, and the latter by injection of one-to-sixty carbolic lotion.

Dr. Bantock and Dr. Champneys kindly assisted me, the latter administering bichloride of methylene. The patient being placed on her back as for ovariotomy, I made an incision four inches long through the parietes, commencing about two inches below the navel. The hemorrhage was very free (even after the main bleeding points had been temporarily secured with Mr. Wells' artery forceps), and the flaccid red intestines lying immediately under the exposed peritoneum rendered the opening of the latter difficult, especially as the afternoon was very dark and foggy. When I had completed the section of the peritoneum, my attention was at once attracted by some small bubbles of gas and a drop of brown fluid on the surface of a coil of intestine. I seized it and sponged it, and found the point of my knife had slightly wounded it. I closed it by a continuous suture of fine silk carried backwards and forwards across the puncture through the peritoneal coat, and for some distance beyond each end of the wound.

This mishap delayed the operation somewhat, and a good deal of intestine was forced out, so that I had to carry my incision an inch higher up, and even then had some difficulty in returning the intestines, the patient not passing well under the influence of the methylene, and keeping up a sort of gasping abdominal respiration. Passing my hand into the abdomen, I found the pelvic portion of the tumour so nearly filling its cavity that I could not dislodge it until I had pressed the mass on the left side through my incision, and used it as a lever by pressing it forcibly over the left iliac crest. This showed how hopeless any attempt to push up the pelvic portion without abdominal incision was. Having at last succeeded in getting the pelvic portion raised into the abdomen, I passed a strong double-string ligature through the left half of the mass just at its junction with the pelvic mass, and having tied each of the strings tightly (taking care that they crossed one another at their point of insertion), I cut away the left half, and thus gained room to transfix the broad ligament on each side with

a double ligature of strong silk—each transfixion being made so that the silks when tied included the base of an ovary with its tube. The ovaries were then cut away—a cyst about the size of a pigeon's egg, which sprang from the right one, bursting during its removal.

The broad ligament on the left side retracted somewhat towards the tumour, and free hemorrhage occurred, requiring the application of a temporary clamp, after the removal of which one or two large vessels were tied separately. Nothing now remained but to deal with the remains of the uterine tumour, and this I did by transfixing the cervix just above the vagina with a curved needle armed with a double ligature of the same silk as I had previously used for the broad ligament. Each half was then firmly tied, and the mass cut away with the knife. No hemorrhage followed, but I thought the stump rather large, and pared it with the scissors; this caused a slight oozing, and I therefore passed one of the ligatures round the whole stump, and securely tied it. Very little blood was lost, and only the pelvis required sponging. I left a sponge over the uterine stump during the introduction of the seven silk sutures with which the incision was closed, and on withdrawing it found it dry, and therefore closed the abdomen. All the ligatures were cut off short, and left with the stumps free in the peritoneal cavity. The operation occupied rather more than an hour and a half, much time being lost from the difficulty in keeping the intestines back after closing the puncture—a difficulty increased by the necessity for handling them very carefully lest anything should be squeezed out between the sutures in the wounded part.

The patient was placed in bed, and the nurse was ordered to keep the room cool, give her nothing but ice by the mouth, and twenty drops of laudanum by the bowel immediately; about fifteen drops of laudanum to be given in this way every two hours or so, in order to keep the bowel perfectly quiet. For the first three hours rather more than this quantity was given, as there was a good deal of pain. She was a little sick at 7.30 p.m. I saw her at 9 p.m.: temperature 99·2°; pulse 92; respirations 16; skin moist and warm; urine scanty and clear. At 3 a.m. she was again a little sick—merely ice-water smelling of methylene. At the morning visit, 9.30 a.m., temperature 99·8°; pulse 96; respirations 20; a good deal of pain across lower parts of abdomen and in hips. At 4 p.m. I saw her again, twenty-four hours after the operation: pulse 112; respirations 26; flatus passing well. At 7 p.m. the nurse found the temperature 101·2°, and put on the ice-water cap to prevent any further rise; it remained on twenty-nine hours, when it was removed, with temperature 98·8°; pulse 88.

The next morning at seven, patient had some headache (temperature 99·6°), and nurse replaced the ice-water cap, and, as the patient found it comfortable, it remained on till the morning of the fourth day, when it was taken off, with temperature 99°. It was replaced at seven the next morning, (temperature 100°), and finally discarded at 1 a.m. on the sixth day. During the first six days there was nothing worthy of note, the stitches being removed on the fourth and sixth days. On the latter nearly the whole wound had healed by first intention.

On the seventh day the dose of opium was gradually reduced in frequency, and it was altogether given up on the tenth day. On the ninth day some red offensive serum came away per vaginam, and this continued more or less till the eighteenth day. On the morning of the eleventh day the bowels were freely relieved after enema. On the evening of the twelfth day she was sick and bilious; and on the thirteenth day I gave three grains of calomel in three doses three hours apart. At night she had great hyperæsthesia of the skin of abdomen; temperature 100·8°; pulse 92. On the fourteenth day the bowels were relieved after a simple enema, but not quite freely; and on the fifteenth day she had a colocyinth, nux vomica, and henbane pill every three hours till they were well relieved.

On the seventeenth day she began to complain of some pain in the right iliac region, and on the eighteenth day the discharge per vaginam ceased, and temperature rose to 100·2°, pulse 92. The discharge returned during the night, and continued going and coming till the twenty-second day, when it quite ceased, and in the evening the temperature was 100·6°, next morning 101·2°, pulse 124 (quicker than it had ever been since the operation.) She had a good deal of pain in the right iliac region, and I found evidences of some fluid accumulation in the abdomen on that side, and behind and to the left of the remains of the cervix, per vaginam. I accordingly placed her on her back in a good light, and introduced a speculum, when I saw a small slough filling up the external os; this I pulled away, and a quantity of fetid pus followed its withdrawal, with immediate relief to her pain. The vagina was well syringed during the removal of the slough and pus, Dr. Bantock again kindly assisting me. Immediately after this the highest temperature since the operation was reached—101·4°. There was free discharge for a few days, but temperature was normal, and pulse about 80. The bowels began to act naturally; the discharge gradually ceased. On February 6, the twenty-seventh day after operation, she was on the couch. She went out on February 16, and went to the Convalescent Home at Eastbourne on February 19.

I examined per vaginam the day before she left, and found a

natural movable os and cervix, the vaginal vault rather hard, especially on the right side; otherwise little to tell of the formidable operation she had recovered from. She had entirely lost her anxious look and wrinkled brow, and was quite a bright, cheerful woman.

I believe this is the first case in which the uterus and both ovaries have been successfully removed, all the pedicles being ligatured with silk and left free in the peritoneum; and a few words are necessary as to my reason for adopting this method in preference to some of those which have proved successful in the hands of Wells, Keith, Péan, and others. I had watched the excellent results obtained in ovariotomy by Mr. Spencer Wells with the silk ligature, even when the stump of tissue left was considerable, and had confirmed my favourable opinion of it by several ovariotomies of my own in which I had used it; and I had also twice seen Mr. Wells successfully remove small fibroid outgrowths from the uterus after transfixing and ligaturing their base with thick Chinese silk.

With such experiences, and having anxiously studied the published cases of Péan and others, I came to the conclusion that the silk ligature was the best thing to use for the pedicles in the case which I have recorded. It has the advantage of being applicable in all cases; whereas Mr. Wells' ovariotomy clamp, the various serre-nœuds and écraseurs, wire, or wire and pins, can only be used in certain special cases. The only one of these methods which it would have been possible to apply in my case would have been a sort of half intra-, half extra-peritoneal method with wire, écraseur, or serre-nœud; and I think the pedicle should be either entirely extra- or entirely intra-peritoneal. I prefer the latter, as I think there is less danger of septicæmic infection; and there is not the danger from hemorrhage when the clamp or wire separates a danger which experience shows to be by no means a small one.

I venture to think this case proves that the silk ligature is to be depended upon, and is worthy of further trial. At the same time, I am fully aware that one successful case of this kind is no good ground for establishing the superiority of one method of treatment; and probably, as in ovariotomy, so in hysterotomy, it will be found advisable to apply different methods in different cases.

I have not alluded to the cautery for division of the pedicle, because I have had no experience with it, and I cannot see that it possesses any advantage over the ligature, and it certainly has several disadvantages. The fact that it is sometimes necessary to ligature some vessels, after removing the cautery clamp, being, to my mind, the most serious objection, as the ligatures are applied to parts of which the vitality is already

much weakened, if not destroyed, by the heat.—*Medical Times and Gazette, April* 7, 1877, *p.* 359.

91.—ON THE RELIEF OF PAIN IN CARCINOMA UTERI.

By Dr. A. E. Aust-Lawrence, Physician-Accoucheur to the Bristol General Hospital.

The subject of the various forms of cancer must always be an anxious care to medical men, inasmuch as the pain in most cases is of such an intense kind that death is welcome to the patients to relieve them of their agony. Considering the great importance of relieving pain in cases where we cannot hope to do more, I have given most remedies a trial; and it is with the object of bringing some of them more prominently before the profession that I am induced to pen a few lines on the subject.

I have, unfortunately, generally under my care in hospital and private practice about from twenty to thirty cases of cancer of the uterus, vagina, or rectum; and the experience of the past twelve months has led me to rely, to a great extent, on the following treatment for the relief of pain:—In cases of medullary cancer of the uterus, and also of advanced epithelioma in the same region, I have been struck with the marked relief often derived from the administration of ergot, in doses of thirty minims every six hours. There is a relief from the intense throbbing, which, as a rule, only subsides with each attack of hemorrhage, which of course brings with it great exhaustion. I consider the ergot acts in the ordinary way, by lessening the amount of blood in the uterus; and it may also check, to a slight extent, the rapid breaking-down of the affected part. A case of medullary cancer in a young woman, thirty-one years of age, was rendered very much less painful by ergot than by any other remedy which was tried. I have a case now under my care of sarcoma of the uterus, the pain of which is very much relieved by full doses of ergot.

Another drug I have found of great value is croton chloral hydrate. This, in my experience, has not very much power to lessen the pain at the seat of the cancer, but it is very valuable in lessening the reflected pains in the back, thighs, and groins; and this it has done in several of my cases to a very marked degree. As a local remedy I have found carbolic acid very valuable. I apply it full strength by means of a little piece of cotton-wool through a very small speculum to the cancerous surface, and then order a lotion with one drachm of the glycerini acidi carbolici to half a pint of water, to be used as an injection night and morning. I have found this drug, used in the way I mention, of great value.—*Medical Times and Gazette, March* 24, 1877, *p.* 310.

92.—THE THERMO-CAUTERY IN OBSTETRIC SURGERY.

By S. W. Iorwerth Joseph, Esq., House Surgeon, London Hospital for Women.

Dr. Paquelin's Cautery (patented), made by Messrs. Collin and Co., of Paris, was figured and briefly described in Lancet, Jan. 20 (see next Article of this volume of the *Retrospect*, p. 353); and some remarks respecting its usefulness in the Royal Infirmary, Edinburgh, were published in the succeeding number.

The frequent use of this cautery at the Hospital for Women permits me to furnish some additional remarks; and, with the permission of the medical officers here, I take the opportunity of bringing before the notice of the profession certain appliances which have been made at their suggestion by Messrs. Mayer and Meltzer, of Great Portland-street, rendering it applicable in a far larger number of cases, and consequently a much more valuable instrument in the hands of the operator. The simplicity, compactness, portability, and efficiency of the cautery are sufficient reasons for recommending it, and the esteem in which it is here held is best conceived when I say that the *fer-rouge* has been altogether discarded, and the galvano-cautery virtually so, being only employed in a few instances.

The class of cases it has been most used for here are necessarily uterine; such as growths of various kinds in the cervical canal; induration, hypertrophy, and carcinomatous growths of the cervix uteri, vulva, and other parts.

As stated in the report above referred to, "some experience is required in regulating the proper heating of the knife, and the amount of pressure with which it should be used, but when this experience is gained the working of the instrument is perfect." Mr. Annandale's observations respecting the heat desired for dividing the several tissues of the body are strictly confirmed by every-day experience in this hospital. When heated to vivid incandescence, large masses of fungating and carcinomatous tissues may be rapidly destroyed; when, on the other hand, it is important to prevent hemorrhage (and a bloodless operation may often be performed), it is desirable that only a red heat should be employed. The regulation of the temperature is easily affected, depending simply upon the manipulation by the hand of an ordinary spray-bellows; by means of which a current of benzoline vapour is made to pass over a heated surface of platinum.

I have myself found the instrument most efficient in arresting hemorrhage. Let me cite two instances. In the first, free hemorrhage was proceeding per vaginam; the case was one of induration of the cervix uteri, to which caustic potash had been

applied by Dr. Heywood Smith on several occasions—the last time four days prior to the bleeding, and the process of ulceration which had led to the separation of a large slough had at the same time laid open an artery on each lip of the cervix, about an inch up the canal. To see the bleeding points it was necessary to separate the lips of the cervix with ovum-forceps, and between the separated blades is inserted the platinum point of the cautery at a dull-red heat. The hemorrhage, which in a short space of time had exceeded two pints, was instantly arrested, which might have been otherwise, judging by the size of the pumping-vessels and their position, had not the cautery been at hand. The second case was one of secondary hemorrhage from the left labium majus, from which an epitheliomatous mass had been removed on the previous day.

While acting so readily and so efficiently as a cautery, the instrument is, in great measure, free from the serious objection of radiating heat, on which account it is peculiarly useful in operative procedures in the mouth, vagina, and rectum Another advantage in it is the platinum head is not so quickly cooled as in most other forms of cautery, it being possible to maintain the platinum hot, when in contact with the tissues, by pumping the benzoline vapour through the handle shaft by constantly manipulating the spray-bellows. Occasionally, however, I have found the platinum head become rapidly cooled, but this I attribute more often to the employment of an inferior quality of benzoline than to any deficiency in the instrument.

The character of the eschar formed varies in appearance, according to the heat at which the cautery is used, and the period of separation is also modified by the nature of the tissue to which it is applied. When employed at a white heat the destruction of tissue is extremely rapid, the eschar formed is generally black, and at this temperature a small zone of surrounding inflammatory redness often occurs; but, as proving the radiating heat to be inconsiderable, I may say that when applied to the cervix uteri a glass speculum is habitually used. As a result of employing a red heat, the cervical tissues assume a white wet leathery sort of appearance; but in both instances the slough which subsequently forms is healthy, and has never as yet given rise to septic absorption.

The separation of the slough is, as a rule, somewhat more rapid, I am inclined to think, than after the use of the galvano-cautery; and occasionally slight secondary hemorrhage occurs, which, however, may always be arrested by a gentle reapplication; indeed, it is seldom necessary to resort to such a heroic measure, a dry plug frequently sufficing. This hemorrhage is possibly due to the short time the slough takes to form being

insufficient for the complete formation of the capillary circulation along the border line of separation; as, however, I have noticed this more generally after operations on the uterus, the nature of the tissue operated upon is doubtless one factor, if not the prime factor, in causing it.

The accompanying diagrams illustrate the additional appliances made for Mr. Reeves and Dr. Heywood Smith by Messrs. Mayer and Meltzer. Each piece may be joined to the common shaft furnished in Collin's instrument.

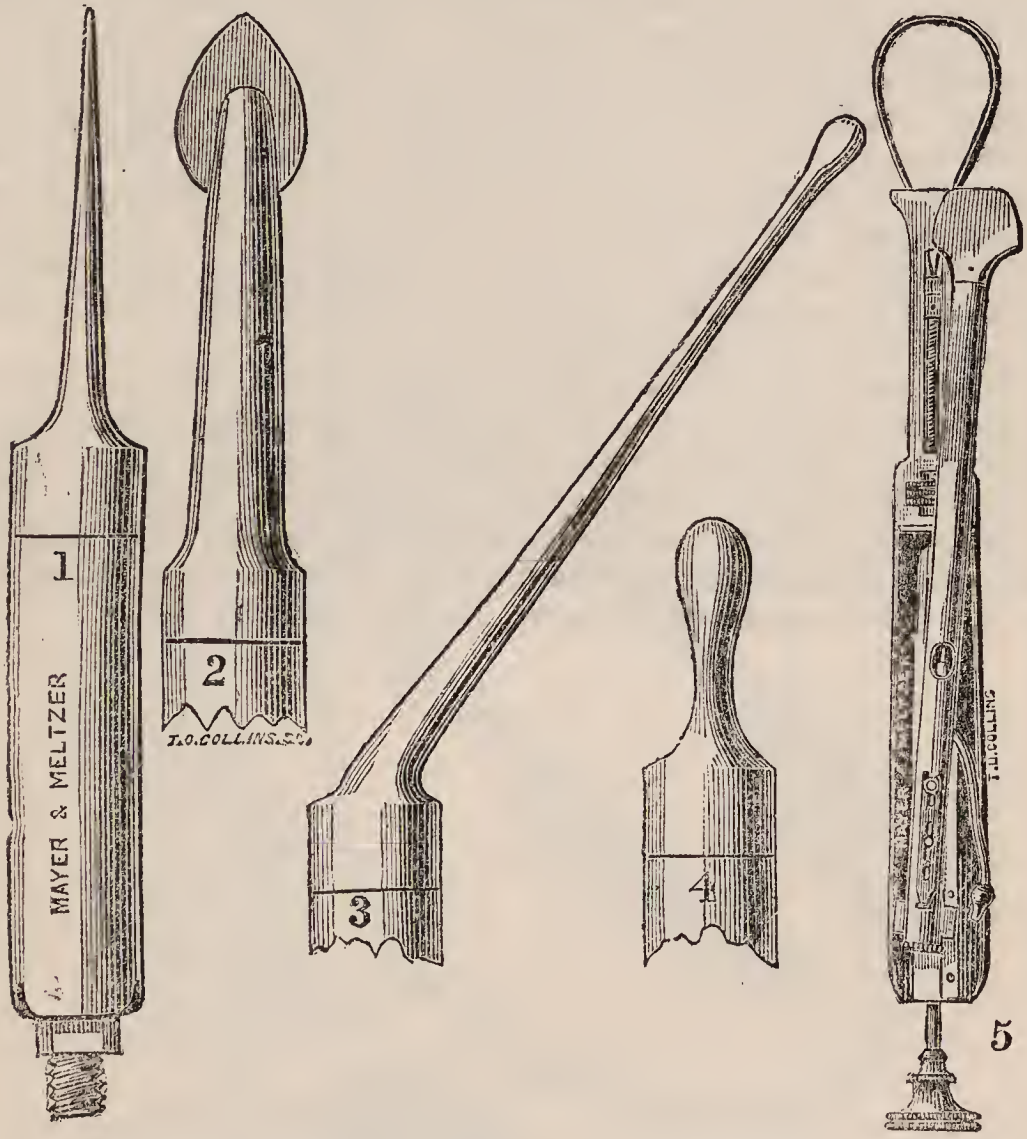


Fig. 1 (made according to the suggestion of Mr. A. Durham) represents a stilette found to be extremely useful for touching ulcers and fissures of the bowel, puncturing internal and external piles with the most satisfactory results, some of the cases so treated leaving the hospital within seven days of the operation (see an article by Mr. Reeves on this subject at page 261 of this volume). It is likewise very useful in ulcerated and granular conditions of the female urethra, also in nævoid growths

and in cases of undue vascularity and vascular growths of the meatus urinarius. Fig. 2 is a knife, very useful for cutting purposes, as in the separation of a pedicle; but especially so in dissecting away growths which have an intimate relation with bone. Fig. 3. illustrates a sound which Dr. Heywood Smith has found very efficient in destroying points of great sensibility in the cervical canal, and for the local treatment of obstinate cases of endo-cervicitis. Fig. 4 represents a thick, round probe, useful for arresting hemorrhage, and in other cases where it is desirable the application of the cautery should be strictly limited to the point first touched. Fig. 5 depicts a form of écraseur and guillotine combined, and though its efficiency is not yet confirmed, it will doubtless prove of great value in many instances, at least it commends itself on examination for the removal of large polypoid growths, &c. The heated platinum plate is propelled forward and at the same time has a lateral movement, by means of a screw apparatus, so that without moving the handle the surface over which the cauterising plate is made to pass is very considerable.

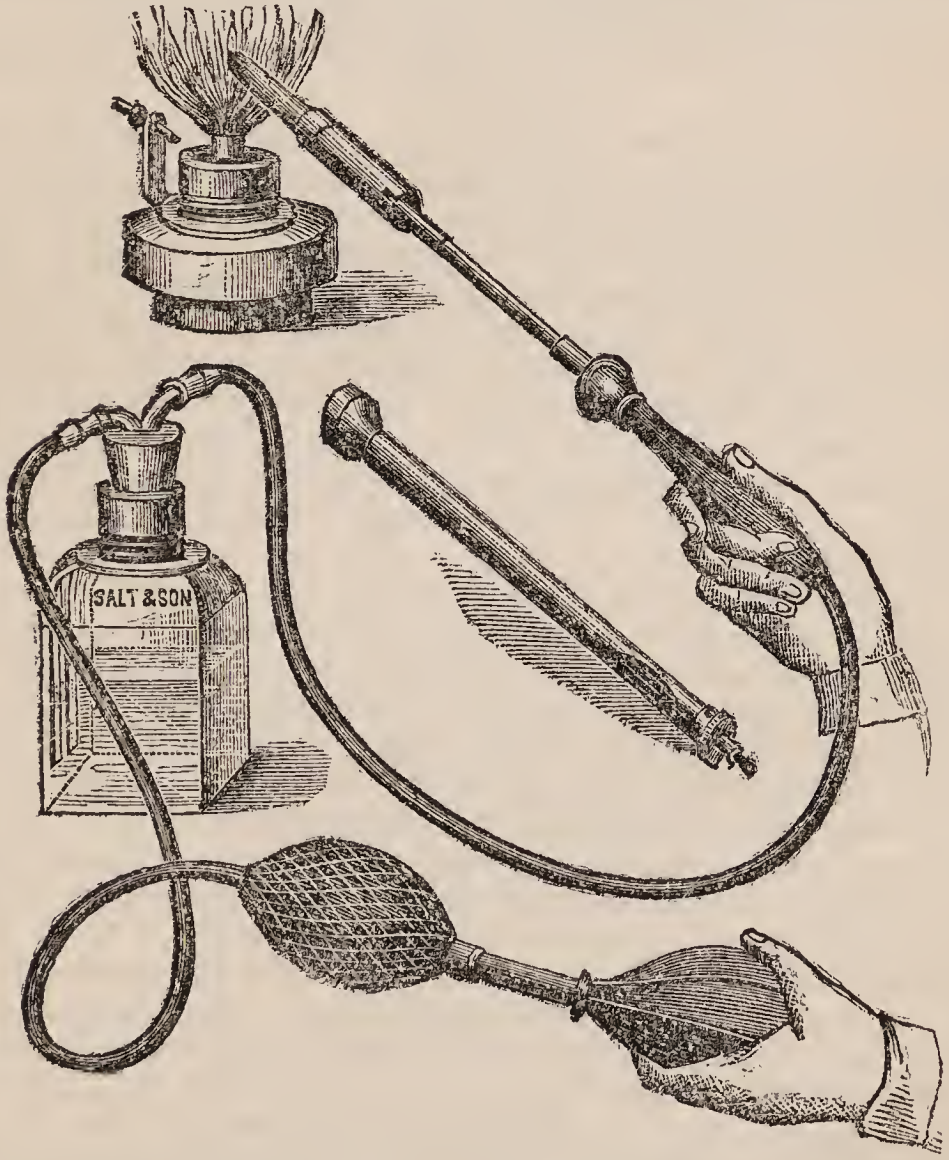
I am permitted to add that Dr. Heywood Smith has applied the thermo-cautery to as many as eight or ten out-patients on one day, so generally useful and so free from injurious sequelæ has it been found. A large number of these cases have been "granular erosion" of the cervix uteri, which, after resisting all other treatment, have readily healed after a few applications of the platinum point.—*Lancet, Feb.* 10, 1877, *p.* 193.

ADDENDA.

93.—DR. PAQUELIN'S NEW THERMO-CAUTERE.

[Messrs. SALT & SON, of Birmingham, have introduced to the notice of the profession, by a woodcut and description in the Lancet, this new and greatly improved mode of applying the actual cautery. Appliances for using with this cautery are described in the preceding article.]

The apparatus consists of a hollow handle, insulated with



wood to protect the hands from the heat, and is furnished with movable platinum heads, corresponding in form to the cautery

irons found generally useful; into these cauteries, which are hollow, after they have been heated to blackness in the flame of a spirit-lamp, a blast of benzoline vapour is introduced by means of an ordinary spray bellows, which at once raises to and maintains them at a state of vivid incandescence. The heat thus produced can be kept up for an indefinite time by slightly compressing the bellows occasionally.

The apparatus is at once simple, handy, cleanly, and elegant, and is packed in a small, neat box containing spirit-lamp, benzoline flask, and cauteries. This apparatus, originally brought out in France, has been recently imported by ourselves. *Lancet, January* 20, 1877, *p.* 108.

94.—THE ADVANTAGES OF ETHER AS AN ANÆSTHETIC: WITH DESCRIPTION OF A NEW INHALER.

By Dr. Lambert H. Ormsby, Surgeon to the Meath Hospital, Dublin.

The power of various substances to produce insensibility has long been known; and in all ages certain narcotic preparations have been employed to produce a more or less unconscious state so as to lull the pain during the performance of surgical operations. It is needless here to enumerate the different anæsthetics employed; their advantages and disadvantages are all doubtless well known. But what seems to interest the profession most on the subject in the present day is the vexed question, What substance have we that combines—1. Safety to the patient; 2. Rapidity in action; 3. Ease of application and portability; 4. Economy of first cost? On each of these heads I would wish to make a few observations.

1. As regards the anæsthetic safest to the life of the patient, few operating surgeons cannot but be aware of the startling fact that week after week, in the columns of the various medical journals, their attention is arrested by a paragraph headed "A Death from Chloroform;" and, taking into consideration the number of deaths attributable to the use of that drug that are never recorded which occur in private practice, a high percentage of mortality must of necessity exist. And even surgeons of lengthened experience who are wedded to the use of chloroform, and who believe that there is no danger in it when properly administered, because they were fortunate enough never to be present at a fatal result during the administration of the drug, must have seen hair-breadth escapes, when dangerous syncopes, pallid cheeks, imperceptible respiration have resulted, and the patient only recovered from the jaws of death by the use of artificial respiration, galvanic battery, ammonia, inversion, and other means resorted to during those apprehensive moments:

a time of the most painful anxiety to the surgeon, to say nothing about the delay in the operation, the appearances to the attendants, friends, &c. Such consequences, I say occur sufficiently often to allow all to be conversant with the fact that, no matter whether a stethoscopic examination of the heart reveals that organ to be free from organic disease, still there is danger; and, from the number of deaths recorded from time to time, the most bold and skilful operator cannot be blind to the fact that in many cases the anæsthetic drug he uses is more dangerous to the patient than the knife he efficiently wields in the interest of surgery. From all that has been written and said on the dangerous effects, my opinion is, that surgeons are not justified, without very good reason, in using a drug that has been proved to be the most dangerous of all anæsthetics. The following statistics, which were proved by the late Professor Morgan of Dublin to be correct, show the relative danger of each agent.

	Deaths.	Inhalations.
Ether	4 to	92,815 or 1 in 23,204
Chloroform	53 to	152,260 or 1 in 2,873
Mixture of chloroform and ether	2 to	11,176 or 1 in 5,558
Bichloride of methylene	2 to	10,000 or 1 in 5,000
Nitrous oxide	Not given.	

From this table, we learn two facts: viz., That chloroform is the most dangerous anæsthetic (of those in ordinary employment) that we could use; 2. That ether is about eight times safer, and proved by the same table to be the safest of all anæsthetics used in prolonged operations.

As regards the advantages of ether, I may mention that I have given it with success to patients varying in age from three months to eighty years in both sexes, and for surgical operations of the most varied nature, comprising deligation of arteries, amputations, delicate ophthalmic and prolonged dental operations. I have kept a young man of twenty-two under the influence of ether, for my friend and colleague Mr. Porter, Surgeon to Her Majesty the Queen in Ireland, for three hours and forty-five minutes, during a prolonged and bold operation for the successful removal of a large pulsating aneurism of the scalp. For my colleague Mr. Macnamara, I have kept a patient for one hour and ten minutes under the influence of ether, during the successful deligation of the subclavian artery in its third stage for aneurism. In this patient, the respiration was considerably impeded by the disease, and yet the ether acted perfectly, with the happiest results.

2. What anæsthetic is rapid enough in action to be universally adopted? In over a hundred ether-inhalations that I have

taken the trouble to tabulate, I find the average time requisite for complete anæsthesia to be produced was two minutes, sometimes less, sometimes more, but in nearly all under three minutes. Few chloroformists can boast of better results. I have administered nitrous oxide gas, and taken it myself; and its action, I am willing to admit, is very rapid and very pleasant, but its effects pass away just as quickly, and are quite inadmissible for prolonged operations of any kind, dental or otherwise.

3. What anæsthetic is convenient as regards portability and application? None more so, I answer, than ether. When kept in a properly stoppered bottle, it is safely carried, occupies a small bulk, and is not liable to spoil; while the inhaler that bears my name can be carried in a remarkably small space, sufficiently convenient in size to be carried in the pocket of the administrator. This, I may mention, is of some importance, for the bulk and unportability of some inhalers have been the insuperable obstacles that prevented them from being more generally adopted. In private practice, this was the great objection to the late Mr. Morgan's inhaler; notwithstanding such disadvantages, however, it has been extensively used in Ireland, England, and America. There is one point, however, which must be borne in mind, that writers in favour of ether have not sufficiently alluded to, and that is the great inflammability of the ether-vapour when brought into contact with flame; and I have seen surgeons who have had to operate at night use reluctantly chloroform instead of ether for that reason. This danger is well to remember; but, with ordinary care, no untoward result may be anticipated, for, by using an inhaler that prevents the ether from escaping, the danger is at once obviated; also, the flame must be brought in actual contact with the condensed vapour before it will ignite.

4. What anæsthetic combines economy of first cost with the three preceding advantages? Many of the efficient forms of ether-inhalers that have been suggested have received less attention than they deserved, owing to their complication, the quantity of ether consumed in each inhalation, and lastly their price, which varies from four to ten guineas, to say nothing about the unportability of the apparatus. Ether is sold on an average at 4s. to 5s. per lb. wholesale, or by retail at sixpence an ounce. The quantity used may, in many cases, be nothing to large institutions: but, in the Army Medical Department, and other places where every sixpence of expenditure is tabulated, this may be of some importance. In the *impromptu* manufactured contrivance, with a towel made into the form of a cone with a hollow sponge in its centre, I have seen six or seven ounces of ether poured again and again during one inhalation with im-

perfect results, air over-diluting the vapour, and the ether at the same time running over the patient's face while the operation was performed and completed, when the patient was merely intoxicated, not anæsthetised.

I maintain, that the one principle that should be kept in view in successful ether-inhalation is to shut air off as completely as possible, and allow the patient to breathe and rebreathe the air already in the lungs. At each respiration, about 25 to 35 cubic inches of air are expired into a thin India-rubber bag, which holds a little more than the ordinary tidal air, so as to make an allowance for any extraordinary expiration. In this bag the condensed ether-vapour is enclosed, which is very soon warmed by the heated air from the lungs, and the vapour is thus prevented from producing any irritating effects on the larynx and air-passages, such as coughing, &c., which cold ether is known to produce. With such an apparatus, which I have been using for the last nine months, I have, with a single ounce of ether, been able to produce complete anæsthesia in less than two minutes; and with children I only use half an ounce of the drug to produce the same effect.

Description of Inhaler and Directions for Use.—It consists of: No. 1. An India-rubber flexible bag, covered over with a network to prevent undue expansion during expiration. No. 2. A soft metallic mouthpiece, which allows of adaptation to any face, the soft metal being able to be bent into any shape: along the border applied to the face it is lined with India-rubber tubing, in order to fit more closely, and thus prevent the ingress or exit of air. Fig. 2 represents a simple sliding valve, so as to admit air if required, or allow its escape if necessary. In the body of the inhaler there is a cone-shaped wire-cage, into which fits a similarly shaped hollow sponge, into which one ounce of anhydrous anæsthetic ether, specific gravity 0.720, is poured, and the inhaler is then ready for administration. If it be necessary to give more ether in a prolonged operation, at fig. 2 there is a tube passing down to the centre of the sponge, into which the ether can be poured without raising the mouthpiece from the face. At first, it is advisable to allow the small aperture at fig. 2 to remain open for a few moments, when it should be closed, so as to allow the patient to breathe and rebreathe the same ether-charged air, until complete anæsthesia is produced.

The annexed woodcut gives a fair idea of the inhaler during administration.

This inhaler has been adopted in many of the leading hospitals in Dublin, viz., the Meath Hospital, the City of Dublin, Dr. Steevens's, and by Mr. Stokes of the Richmond Hospital. When last in London, by the kindness of Mr. Christopher Heath (and I take this opportunity of returning him my best thanks), I was permitted to demonstrate its use on a patient of his at University College Hospital, and Dr. H. Radcliffe Crocker, Administrator of Anæsthetics to the Hospital, says of it, " that while extremely simple, portable, and easily managed, it was thoroughly efficient in its action, the patient being rendered completely insensible in two minutes, without coughing or unpleasant symptoms, while only a single ounce of ether was consumed in an operation occupying about ten minutes."

This inhaler is made by Messrs. Coxeter and Son, Grafton Street East, London, who deserve my best thanks for the efficient manner in which they have carried out my directions, besides bringing it out at the very moderate price of 24s.—*British Medical Journal*, *April* 14, 1877, *p.* 451.

95.—ON THE INHALATION OF NITROUS OXIDE GAS.

By Dr. George Johnson, F.R.S., Professor of Clinical Medicine and Senior Physician to King's College Hospital.

In the phenomena which attend the inhalation of *nitrous oxide gas*, when given as an anæsthetic, we have a very interesting confirmation of the results obtained by experiments on animals, and, on the other hand, the records of physiological experiments enable us more completely to understand and interpret the facts of nitrous oxide anæsthesia. On several occasions, I have availed myself of the opportunity afforded me by the courtesy of the authorities at the Dental Hospital to watch the phenomena which attend the inhalation of the gas, and I will now briefly describe them.

In most cases, during the first few seconds the pulse and the breathing are quickened, as a result probably of emotional excitement. In the next stage, the breathing becomes slow and shallow and the pulse full and firm. Then, after a period which varies in different cases from forty to eighty or ninety seconds, the pulse suddenly becomes almost, or even quite imperceptible, the features become livid, the pupils are widely dilated, there is a state of general muscular rigidity; in short, all the phenomena of the first stage of an epileptic fit are present. The mouth-piece being removed, the morbid phenomena quickly pass away, the features regain their normal colour, the pulse

returns, and for a few seconds has again a full and throbbing character, but quickly regains its normal condition.

The explanation of the phenomena appears to be sufficiently obvious. It is admitted on all hands that, at the temperature of the body, the nitrous oxide gives up no oxygen to the blood or the tissues. The gas becoming rapidly diffused and replacing the oxygen in the lungs and in the blood, black unoxygenised blood passes into the systemic arteries, and excites, through the vaso-motor nerves and centre, contraction of the muscular arterioles. The resistance thus offered to the passage of unaërated blood through the terminal arteries explains the temporary fulness and tension of the radial pulse. The unoxygenised blood, passing through the systemic capillaries without the usual interchange of materials between it and the tissues, returns to the lungs in an abnormal condition, and there excites contraction of the pulmonary muscular arterioles. The resistance thus offered to the passage of blood through the lungs explains, on the one side, the systemic arterial emptiness with feebleness or even complete disappearance of the pulse, and, on the other, the systemic venous fulness with lividity of the skin. The epileptiform condition is explained by the sudden and extreme diminution of the blood-supply to the brain, the blood at the same time being unaërated.

If the inhalation were continued, death would occur from the complete arrest of the pulmonary circulation and consequent overdistension of the right side of the heart, and this is the mode in which death occurs when an animal is killed by the continued inhalation of the gas. A year ago, my friend and colleague Mr. Hamilton Cartwright assisted me to kill two rabbits with the gas. In both animals, convulsions preceded death; and, the chest being opened immediately after death, the heart was found still beating. The right cavities and the systemic veins were greatly distended with blood, while the left cavities and the aorta were comparatively empty and flaccid; the blood on both sides of the heart being equally black. The lungs were anæmic and collapsed to an extreme degree.

It will be seen that the phenomena observed during life and the appearances after death from the inhalation of the nitrous oxide gas are precisely similar to those which result from suspension of the respiration in the human being and in the lower animals.

It is evident, from the many thousands of cases in which the gas has been given and the extreme rarity of a fatal accident from its use, that, in the hands of a skilled and careful operator, no great risk attends the employment of this anæsthetic; but it is also obvious that, to a patient with a feeble fat heart, the distension of the right cavities which accompanies the disappear-

ance of the radial pulse and the general lividity of the features must be attended with some degree of risk, and the danger must be increased when, the muscles of the trunk and limbs being convulsed, the pressure of the contracting muscles upon the veins drives the blood forcibly towards the right cavities of the heart, and so adds to their distension.—*British Medical Journal, April* 14, 1877, *p.* 446.

96.—PORTABLE REGULATING ETHER-INHALER.

By J. T. Clover, Esq.

[In our last volume of the *Retrospect* (p. 364), Mr. Clover's apparatus for giving laughing-gas and ether, separately or combined, was described. He now further says:]

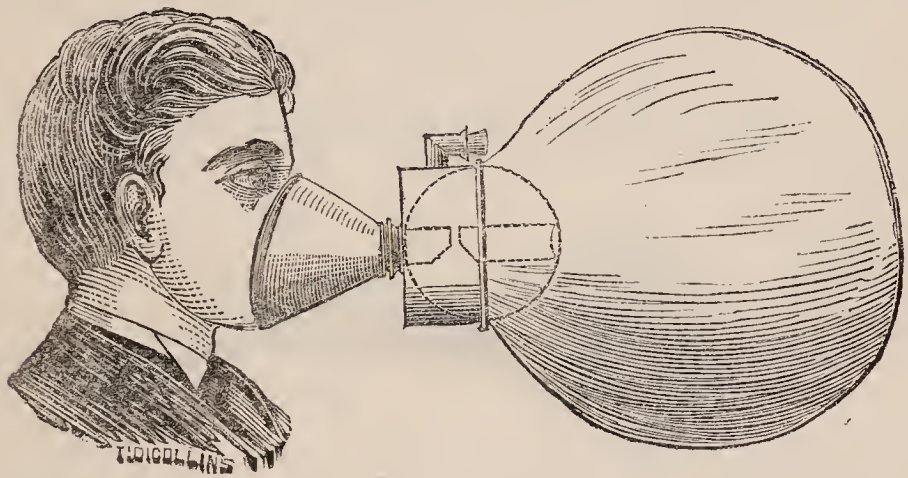
Experience in more than three thousand cases in which I have used it convinces me that the administration of ether may be made far less unpleasant to the patient, and equally effective and safe, by first giving enough gas to render the patient unconscious of its taste.

The arrangement of the apparatus enables one to cause the patient to breathe directly into and out of a bag, or partly, or entirely through a vessel containing liquid ether; and, even without gas, it is very efficient, inasmuch as it gives the power of varying and of sustaining the strength of the vapour. I have used it a great many times without gas, and find it as safe as any other way of giving ether, whilst the risk of coughing and sickness is much lessened.

The plan of excluding fresh air until insensibility has been induced, and admitting it very sparingly afterwards, has now been extensively tried in various ways, and, so far as I know, it is practically free from the danger of causing serious obstruction to the pulmonary circulation and over-distension of the right cavities of the heart. Of course, air cannot be indefinitely excluded, but the pulse and respiration give timely notice when air is required. A single artificial respiration of fresh air in these cases affords more relief than several such respirations when the apnœa has resulted from an overdose of ether or chloroform. The reason for this is, that in the former case the symptoms depend chiefly on the want of oxygen, and in the latter upon the presence of a substance which has not only entered the blood, but has penetrated the tissues of the body. If the apparatus be overheated, or if the ether be turned on too quickly, the ordinary coughing and struggling would, of course, be produced. The apparatus, however, requires a little more attention to temperature and other details, and is rather too complicated for general use. I have made several attempts

to avoid the necessity of warming it. This can be effected by having the ether-vessel surrounded by a larger quantity of water at the ordinary temperature; but then the size and weight of the inhaler become objectionable. Better success attended modifications of the instrument having the ether-vessel placed close against the face-piece, so as to receive more warmth from the patient's breath and from the hand of the administrator.

I am greatly indebted to Messrs. Mayer and Meltzer (instrument makers, 71, Great Portland Street, London, W.) for their patience and ingenuity in carrying out my ideas, and my present object is to call attention to a portable regulating inhaler made by them. Its advantages are these:—1. It has no valves; 2. It supplies the vapour so gradually, that patients breathe quietly; 3. It produces sleep in two minutes; 4. It



does not require fresh ether during the continuance of an operation; 5. The recovery from a short operation is more speedy than with most other inhalers; 6. It does not need to be warmed before it is used; 7. No sponge or felt is required; 8. Ether left in the inhaler can be saved for another time.

The face-piece is edged with an air-cushion. The ether-vessel and water-chamber rotate upon the mount of the face-piece. When the instrument is first applied, the stopper should be towards the patient's forehead, and now he breathes in and out of the bag directly. As the ether-vessel is turned round, the air is obliged to enter the ether-chamber and pass through it before it reaches the bag; and, when the vessel is turned half-round, so that the stopper is opposite the patient's chin, all the air going in and out of the bag must pass through the ether-vessel. Two ounces of ether (specific gravity 735) are enough for a long operation. Usually, an ounce and a half is the proper charge. The opening for supplying the ether is arranged to prevent an excessive quantity being supplied; but, to guard against the possibility of a few drops escaping through

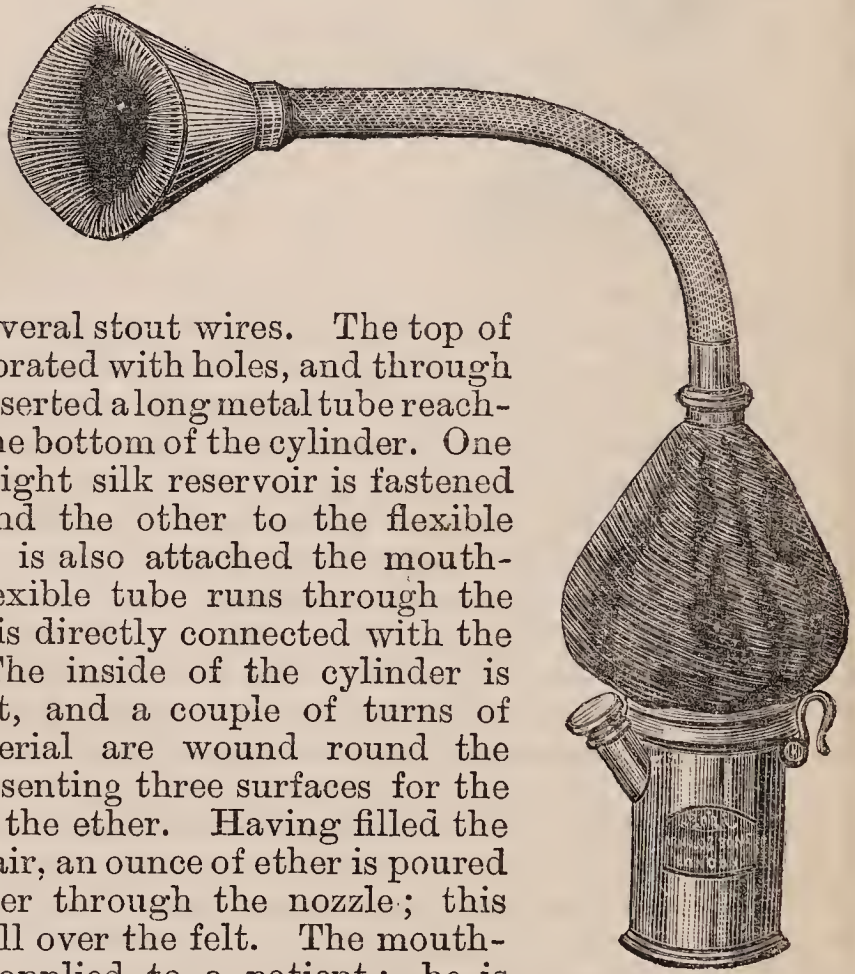
the inner openings, there are two recesses made to catch them, and prevent the liquid ether from reaching the patient's lips.

The ether-vessel is spherical in shape, and one-half is surrounded by a closed water-compartment, to prevent the ether from becoming too cold. The bag need not be so much distended when in use as is shown in the woodcut, and can be kept on one side so as not to obstruct the light in operations on the eye. The instrument is intended for giving ether without gas; but, by connecting the bag with a supply of nitrous oxide, it forms a tolerably efficient substitute for the gas- and ether-inhaler above mentioned.—*British Med. Journal, Jan.* 20, *p.* 69.

97.—DESCRIPTION OF A HANDY ETHER-INHALER.

By W. T. ANGOVE, Esq., House-Surgeon Yarmouth Hospital, late Administrator of Anæsthetics West London Hospital.

As there is a great want, just at present, of an effective, handy, and cheap ether inhaler, I have had one made, which seems to answer every purpose, and which may perhaps be of some use to the profession in general, but especially to private practitioners.



It consists of a cylinder, on to which fits, by a bayonet-joint, a cap, around the rim of which are attached several stout wires. The top of the cap is perforated with holes, and through the middle is inserted a long metal tube reaching nearly to the bottom of the cylinder. One end of an air-tight silk reservoir is fastened to the cap, and the other to the flexible tube, to which is also attached the mouth-piece. The flexible tube runs through the reservoir, and is directly connected with the metal tube. The inside of the cylinder is lined with felt, and a couple of turns of the same material are wound round the wires, thus presenting three surfaces for the evaporation of the ether. Having filled the reservoir with air, an ounce of ether is poured into the cylinder through the nozzle; this suffuses itself all over the felt. The mouth-piece is then applied to a patient; he is

told to "draw in" his breath; the vapour he inspires comes from the reservoir, passes through the holes in the cap, over the evaporating surfaces of felt, and up through the whole length of tube; he expires the same vapour, which passes back to the reservoir and becomes recharged with ether during the next inspiration.

I have completely anæsthetised several individual patients in a little over one minute, one in forty seconds. An ounce of ether is sufficient to keep a patient under about ten minutes.

The apparatus is very handy, the cylinder being only about five inches high and three in diameter. The reservoir holds about a pint and a half. The length of the tube can be made according to taste.

Messrs. Arnold and Sons, of 35 and 36 West Smithfield, London, made mine, and are ready to supply them at a comparatively small cost.—*Lancet, Dec.* 16, 1876, *p.* 853.

98.—ON A MODE OF GENERATING SULPHUROUS ACID FOR USE AS A DISINFECTANT, &c.

By Thomas W. Keates, Esq., Consulting Chemist to the Metropolitan Board of Works.

From the remotest time, burning sulphur has been employed to fumigate and purify infected air, and to destroy fermentative and putrefactive action. There is no agent more powerful in its effects than this. Unlike chlorine, it not only acts as a disinfectant or destroyer of disease-germs and of the results of putrefaction, but it is also a powerful preservative agent, and, like carbolic acid, is a preventive of chemical changes in dead organic matter of every kind.

Although the value of sulphurous acid is thoroughly understood, its use is necessarily limited by the difficulty which exists in the way of producing it in a form in which it can be readily applied. The ordinary method of generating it by burning sulphur is cumbrous and very uncertain, owing to the difficulty of keeping up the combustion; there are also many situations in which the process cannot be carried on at all, and under the best circumstances it is inconvenient and but little under control. The evolution of the gas from its solution in water is scarcely more convenient, while it is much less effective; indeed, it may be said that there is no ready, convenient, and easily controllable way of producing this valuable agent in use at present; and this is the more remarkable when it is considered what a ready and simple means we really have at hand for this purpose.

Most readers are no doubt familiar, at least theoretically, wit

the substance called bisulphide of carbon. This is a compound of one atom of carbon with two atoms of sulphur (C,S_2); it is a dense, mobile liquid, heavier than water, and intensely inflammable, burning in the air like spirit of wine. During combustion the constituents of the bisulphide combine with the oxygen of the air, producing sulphurous and carbonic acid gases; but as 100 parts contain, by weight, as much as 84 parts of sulphur, which will give, in burning, 168 parts of sulphurous acid, it will be seen that the volume of this gas from a given quantity of bisulphide greatly exceeds that of the carbonic acid, and is comparatively very large. Suppose the above quantities to be in grains: as 100 cubic inches of sulphurous acid weigh 68·5 grains, the 168 grains will measure upwards of 245 cubic inches, or about one-seventh of a cubic foot, which is the volume of sulphurous acid obtainable from 100 grains of bisulphide.

The bisulphide of carbon can be burned in a common spirit lamp, and in that case the products are sulphurous acid and carbonic acid only, in relative proportion to the atomic composition of the bisulphide, as I have stated; but by a modification of the method of burning, the amount of sulphurous acid produced in a given time can be regulated to any desired extent.

It is a property of the bisulphide of carbon to dissolve in fat oils and hydrocarbon liquids, such as petroleum; so by mixing it with any one of these liquids and burning the mixture in a properly constructed oil or petroleum lamp, sulphurous acid will be generated with the other usual products of the combustion of such materials, and in proportion to the quantity of bisulphide present in the mixture of combustible liquids: any proportionate quantity of sulphurous acid can in this way be thrown into an atmosphere, and the action may be continued for any length of time.

As the sulphurous gas is generated *pari passu* during the combustion of the bisulphide, it diffuses itself in the air, which in a short time will become completely impregnated with it. In a room containing about 1300 cubic feet of air it was found that by burning 280 grains of the bisulphide the atmosphere was so far charged with sulphurous acid that it was impossible to remain in the room for more than a few seconds. In five minutes after the lamp was lighted litmus paper began to be reddened at some distance from it; in ten minutes the air had become very oppressive, and the litmus paper was reddened in the extreme corners of the room; in fifteen minutes the air was so charged with the gas that it could scarcely be breathed, and in twenty minutes it was unbearable. In that time, as I have said, 280 grains of bisulphide were consumed in a simple single-wick candle.

Sulphurous acid generated in this manner can be applied with facility to the disinfection of any place or object. In the case of rooms in which infectious or contagious disease has prevailed, it is only necessary to light the lamp and allow it to burn until the atmosphere has become impregnated with the gas to any desired extent, and then to remove or extinguish it just like a common spirit-lamp. In the simple form of apparatus which I suggest for this purpose, the lamp is enclosed in a metal case, about three inches in diameter and eight or nine inches high, furnished with holes near the bottom for the admission of air, and others in the top for the emission of the sulphurous gas. (This apparatus is made by How and Co., St. Bride's street, Ludgate-circus.) This can be conveniently moved about, and placed, while the lamp is burning, in almost any locality. Receptacles for infected clothing, or the clothes or linen used in connexion with disease, or carriages which have conveyed fever or other patients, can be thoroughly purified without difficulty and with very little trouble. For the disinfection of ships, too, the lamp is particularly suitable, as it can be carried into the remotest part of a ship and burned without the least danger, and with the certainty of effecting its object completely.

It must be observed that the bisulphide of carbon is extremely volatile, having its boiling point as low as 110° F.; it is therefore necessary that the lamp in which it is burned should be furnished with a well-fitting screw-cap, to prevent the liquid from evaporating, and at the same time to keep its peculiar odour from escaping. This odour is often very nauseous, but the bisulphide is now manufactured by Messrs. C. Price and Co., of Thames-street, so pure, that it possesses very little smell, and can be used without the least inconvenience.—*Lancet, Nov.* 18, 1876, *p.* 712.

99.—ON THE EXTERNAL USE OF SALICYLATE OF IRON.

By Dr. Robert Kirk, House Surgeon, Royal Infirmary, Edinburgh.

Salicylic acid and its compounds have been used for some considerable time as antipyretics and antiseptics, and as such have fulfilled very satisfactorily what was demanded of them; but still they had the disadvantage, shared also by carbolic acid, of being in no way astringent, and therefore of allowing free capillary bleeding after operation, unless pressure was employed. An astringent antiseptic seemed therefore to be desirable, and after experiments with a number of salts, trial was made of the salicylate of iron, which seems to have both actions in an almost equal degree.

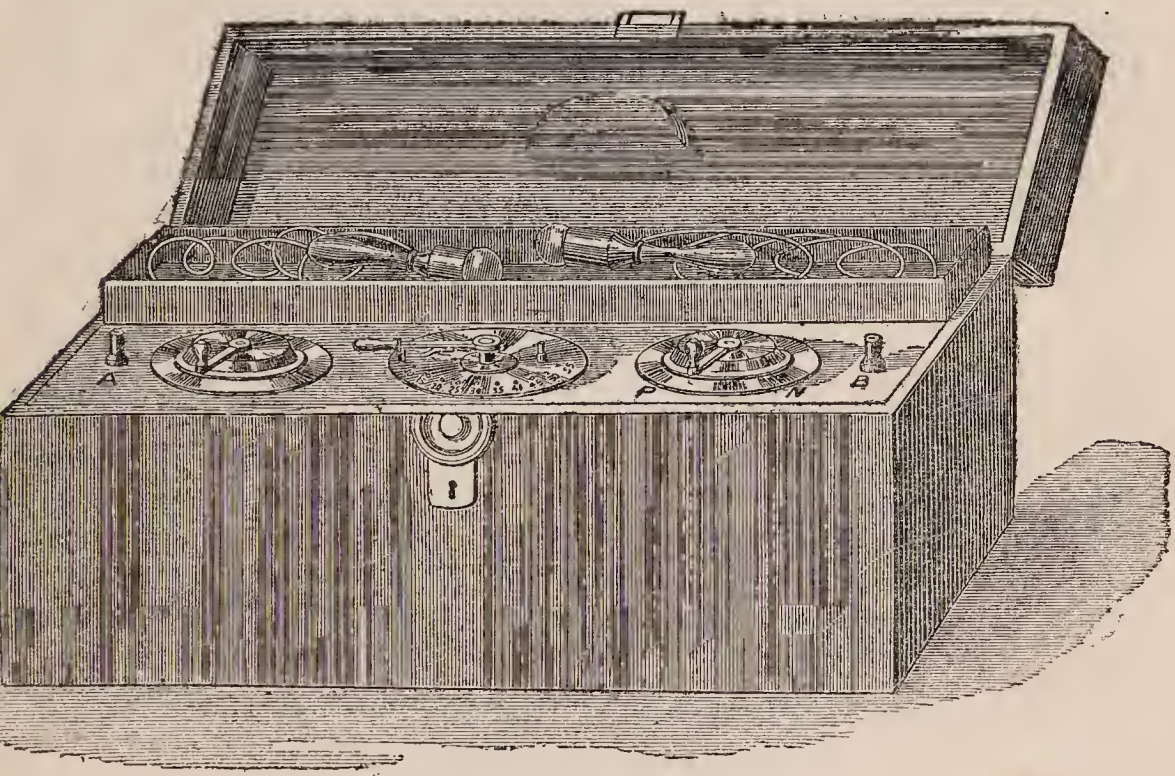
By adding salicylate of soda to a saturated solution of sulphate of iron a double decomposition takes place, and sulphate of soda and salicylate of iron are obtained in solution, from which the latter may be easily separated by crystallization. This is, however, quite unnecessary, as the solution just mentioned is a most convenient form for application, and one which can be prepared in a few moments. Of a bright claret colour, with no smell, and with hardly any irritant properties even when used in strong solutions, it is not so repulsive as some of the more generally used disinfectants and antiseptics. Its antiseptic properties are most easily demonstrated, for urine containing but a small quantity of this salt in solution will remain long free from bacteria when exposed to the same influences as those which are conducive to the life of bacteria generally.

The cases to which this salt has been applied have generally been open sores, often with more or less unhealthy action going on, but in each the progress towards recovery has been very rapid after the first few days. As an illustrative case I may be allowed to give the following :—

Sarah W., age 67, was admitted into Ward 1, Royal Infirmary, under the care of Mr. Joseph Bell, on 31st October, suffering from two varicose ulcers on the right leg. The larger ulcer, about the size of half-a-crown, was situated midway between the ankle and knee on the inner side of the limb; and the smaller, the size of a florin, was about two inches lower. The surrounding parts were inflamed, while the ulcers themselves were devoid of granulations, and painful. Rest was at once enjoined, and black wash was employed until the end of November, when the salicylate of iron was employed instead. The ulcers were then as large as florins, and still were almost free from granulations. For the first three days after the solution was employed, there was but little evident progress made as far as the size of the sores was concerned; but the surfaces became more healthy, granulations formed, and the edges became softer. Cicatrization now followed rapidly, a decided difference being noticed every day, and the patient was dismissed on 13th December, the sores being completely healed, although the skin surrounding them was covered with a red crust, which, however, was rapidly removed with tepid water. The dressing used in this case was that of lint soaked in the solution, and covered with guttapercha tissue. Were I disposed, I could easily multiply cases of cures as rapid as that quoted, but I do not think this at all necessary, as the case, I think, speaks for itself. — *Edinburgh Medical Journal, Feb.* 1877, *p.* 707.

100.—THE PATENT LECLANCHE BATTERY.

The increasing use of electricity as a therapeutic agent brings with it gradual but sure improvement in the appliances for producing that agent. The Leclanché battery, without doubt, possesses the greatest perfection hitherto attained in the production of medical batteries. Each cell occupies the space of about two fluid ounces, is made of ebonite, and sealed at the top. The Leclanché cell consists of a rod of zinc, immersed in a concentrated solution of chloride of ammonium, a porous diaphragm separates from the zinc a rod of gas carbon, surrounded with coarsely-powdered gas carbon and native peroxide of manganese. The cells are usually arranged in polished teak cases, containing from twenty to sixty, for convenience of manipulation and carriage, and connected in series of five, and are so arranged that the strength of the current can be readily increased or diminished by the strength of five cells. As seen



55 cell Leclanche Battery.

in the illustration, they are fitted with a current-breaker, a commutator (the centre dial, for increasing or diminishing the strength), and a current-reverser, the operator being thus enabled to administer the current in an intermittent form, or to change the direction of the current instantaneously.

When put to ordinary medical use, these batteries are warranted to remain in perfect condition for two years, or even a longer period without attention.

The electro-motive force of the Leclanché cell is equal to about 1·5 that of an ordinary Daniell element, or in other words, 10 Leclanché cells are equal to about 15 Daniell, whilst its internal resistance is very considerably less, and consequently the quantity of electricity generated is much greater, being in round numbers ten times that of the Daniell.

For durability and cleanliness the patent Leclanché cells are unequalled, requiring but a minimum of attention; whilst on the other hand, for cost of working they are the cheapest form of battery that can be employed, as, when not in actual use, there is no local action taking place, the consumption of material being in direct proportion to the amount of work done.

Each battery is supplied with a pair of rheophores of the most approved kind, and each box has plain directions for use affixed in the lid.

The Leclanché Battery has already been adopted by some of the London hospitals, and we are informed by the manufacturers that they have sent them to many provincial and foreign hospitals.

The sole licensees and manufacturers for the United Kingdom are the India-rubber, Gutta-percha, and Telegraph Works Company, 100, Cannon-street, London, E.C.—*Lancet*, *Jan.* 6, 1877, *p.* 17.

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